Infinite Energy

Cold Fusion and New Energy Technology

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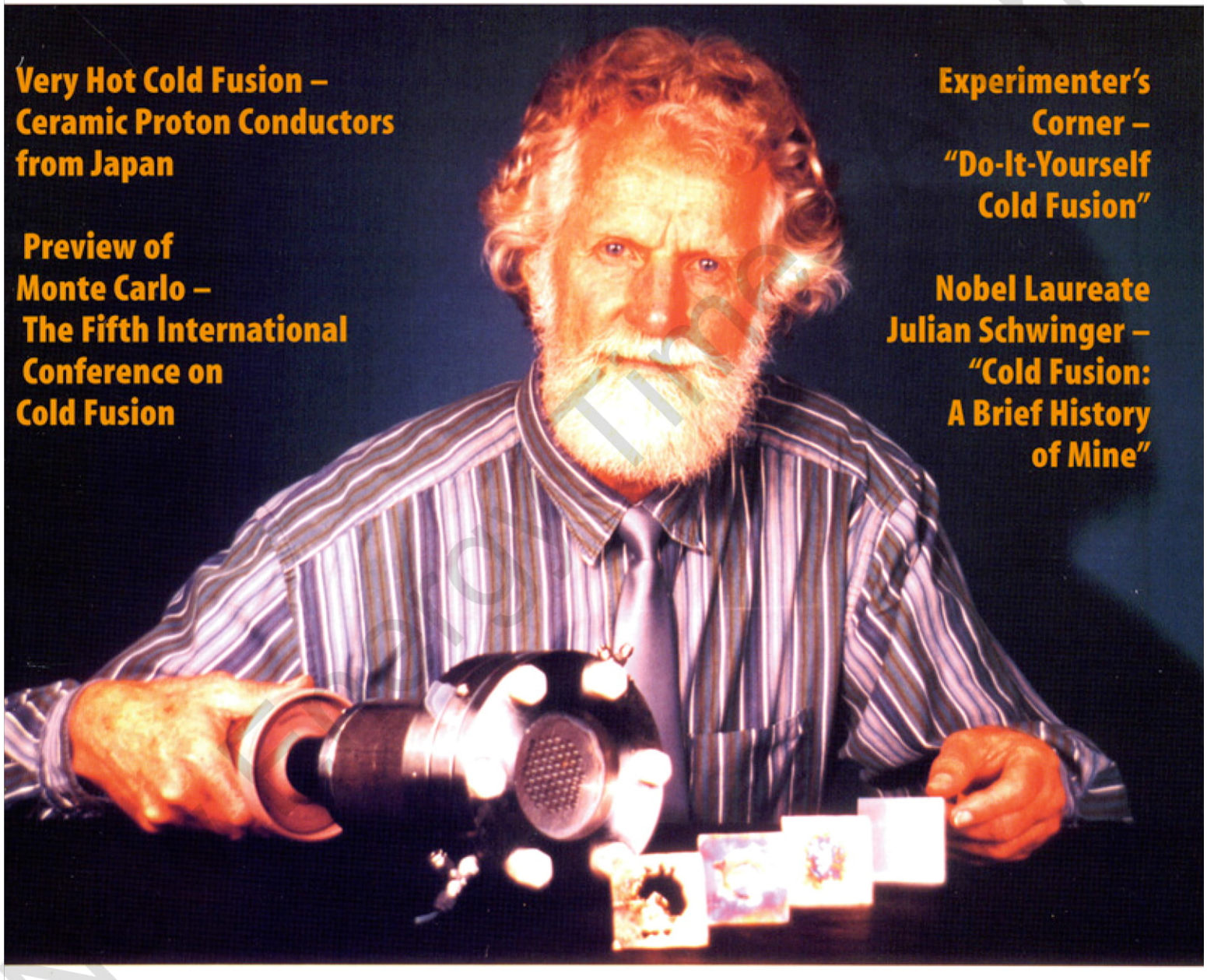

Very Hot Cold Fusion – Ceramic Proton Conductors from Japan

Preview of Monte Carlo – The Fifth International Conference on Cold Fusion

Experimenter's Corner – "Do-It-Yourself Cold Fusion"

Nobel Laureate Julian Schwinger – "Cold Fusion: A Brief History of Mine"

Table-Top Alchemy: Roger Stringham's Cold Fusion Reactor Creates Helium-4 and Lots of Excess Heat

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Welcome to the Fifth International Conference on Cold Fusion (ICCF5)!

ICCF 5

9–13 April 1995, Monte Carlo, Monaco at the Loews Hotel.

To those attending the conference and to those who are here only in spirit, eagerly awaiting the latest results on the scientific frontiers of "Cold Fusion" and New Energy Technology, we wish you well. Welcome, indeed, to the New Energy Age! —The Editors of *INFINITE ENERGY* ∞

Tentative Program of ICCF5

9 April, Sunday

Registration Confirmation	14:00-20:00
Welcome Reception	19:00-20:00

10 April, Monday

Session 1 Introduction

Welcome	8:30-8:35
A Critical Overview of Cold Fusion	8:35-9:30

Session 2 Excess Power Generation

Plenary Lecture	9:30-10:00
Oral Presentations	10:00-10:40
Coffee Break and Posters	10:40-11:00
Oral Presentations	11:00-12:30
Lunch and Posters	12:30-14:00

Session 3 Excess Power and Calorimetry

Oral Presentations	14:00-15:20
The Experimenters' Regress	15:20-15:40
Coffee Break and Posters	15:40-16:00
The Experimenters' Regress	16:00-17:00

Session 4 Nuclear Measurements

Plenary Lecture	17:00-17:30
Oral Presentations	17:30-18:00

11 April, Tuesday

Session 5 Nuclear Measurements, Nuclear Ash

5.1 Tritium

Plenary Lectures	8:30-10:00
Oral Presentations	10:00-11:00
Coffee Break and Posters	11:00-11:20

5.2 Neutrons

Plenary Lecture	11:20-11:50
Oral Presentations	11:50-12:30
Lunch and Posters	12:30-14:00

5.3 Helium and Other Signatures

Oral presentations	14:00-16:00
Coffee Break and Theory Posters	16:00-16:20

Session 6 Theory and Modeling

Plenary Lecture	16:20-17:00
Oral Presentations	17:00-18:00

12 April, Wednesday

Session 7 Theory and Modeling

Poster Session	8:30-9:30
Plenary Lecture and Summary of Posters	9:30-10:20
Discussion Session	10:20-10:50
Coffee Break and Posters	10:50-11:10

Session 8 Cathode Loading and Materials

Plenary Lecture	11:10-11:40
Oral Presentations	11:40-12:30
Lunch and Posters	12:30-14:00

Session 9 Cathode Loading and Materials

Plenary Lecture	14:00-14:30
Oral Presentations	14:30-16:00
Coffee Break and Posters	16:00-16:20

Session 10 New Developments

10.1 New Approaches

Oral Presentations	16:20-18:00
Conference Banquet	20:30

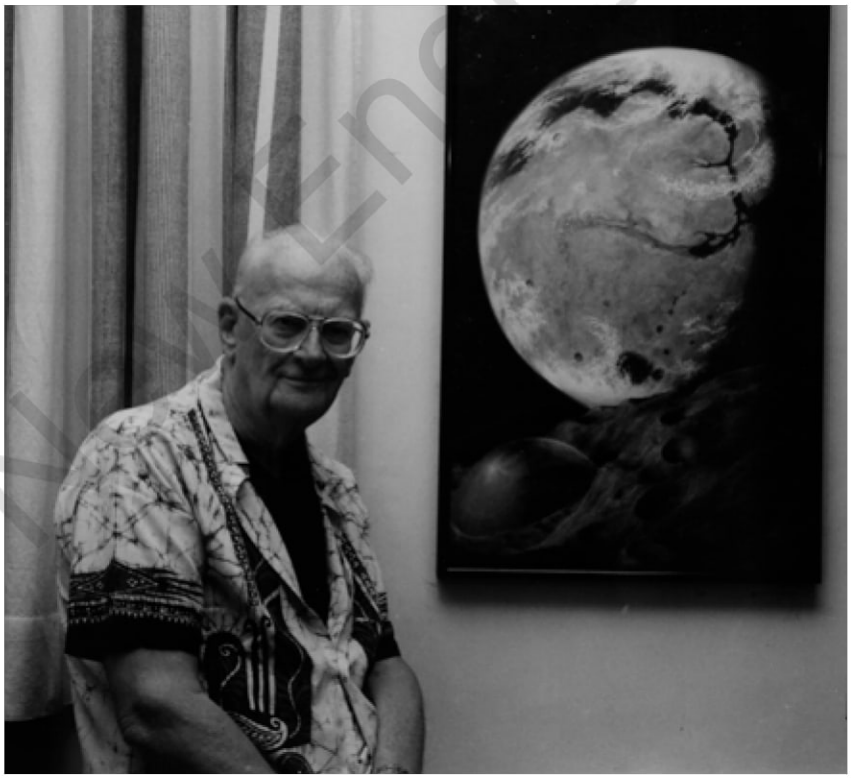
13 April, Thursday

10.2 New Systems

Oral Presentations	8:30-10:00
Coffee Break and Posters	10:00-10:20

10.3 Charting the Way Forward

Oral Presentations	10:20-12:00
Highlights and Conclusions	12:00-12:30



Welcome to the Apocalypse?

I am a little embarrassed to recall that exactly two years ago, I addressed a distinguished gathering of American, Russian, and other naval staff officers (including the OIC, U.S. Pacific Fleet) on the subject of "Cold Fusion" —and hinted that the breakthrough would be 'real soon now.'

Well, I'm still waiting in 1995, but I sincerely hope that after Monaco, both the hand-waving and mud-slinging will finish, though a little egg-throwing may still be in order.

Anyway, I congratulate Gene Mallove on his courage in launching INFINITE ENERGY and though his title may be criticized on logical grounds, I really can't think of a better one.

But suppose the energy bank is broken at Monte Carlo—what then? How much thought has been given to the awesome geo-political and financial repercussions? We've seen what's happened to the world's currencies in the last few years...I'm scared, as well as excited.

Yet at least one good thing may come of it. I am tired of seeing hordes of lunatics on TV every night, screaming and waving pieces of paper at each other in the money markets. Perhaps when we can synthesize gold by the ton, they can be diverted to less ulcer-generating and more useful jobs, like garbage collecting and sewer maintenance.

Arthur C. Clarke, CBE
Columbo, Sri Lanka, 20 March 1995

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Infinite Energy

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COVER: Roger Stringham of E-Quest Sciences with his Micro-fusion Reactor and metal foils melted under water. *Photo: Steve Wolff, Affordable Photography*

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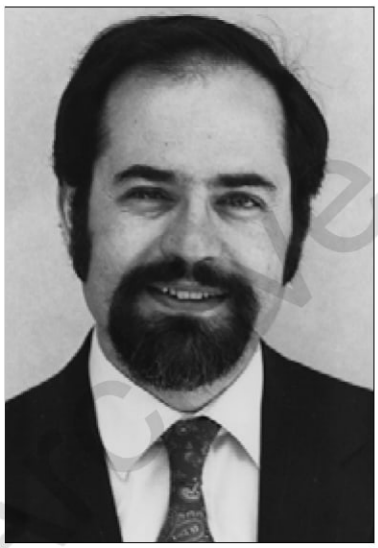
By Eugene F. Mallove, Sc.D.

Why "*Infinite Energy*"?



Every new magazine needs to explain itself on "Day One," and we admit that this one needs some explaining! Infinite Energy? Sounds preposterous, doesn't it? Perhaps, until you examine what has been happening in the so-called "cold fusion" field since 1989— and in the "New Energy Technology" field, which actually began many years ago when scientists, tinkerers , and inventors were puzzled by unexplained anomalies of excess power production in heat-producing as well as in electricity-producing machines.

Are these phenomena related —cold fusion and other forms of excess power? More to the point, are they real at all? Read our magazine and draw your own conclusions. That's all we ask of you. Be true scientists and study the data without jumping to the rash conclusion that preconceived theories rule out the mountain of "cold fusion" and "new energy technology" data that will confront you. We would be surprised if you did not come to agree with us that the excess heat and excess power phenomena are both real and revolutionary. Of course, not all experiments in this dynamic field may be said to be conclusive, but the overall range and multiplicity of findings leaves no doubt, in our minds at least, that new science and a rapidly emerging new technology are upon us.

It's 1995. We say, welcome to the New Energy Age! Chemists, engineers, and physicists: you have a big wake-up call, 100 years after Roentgen's discovery of x-rays (November 1895) revolutionized classical physics.

It is difficult to imagine a more profound reversal of scientific fortunes than what has been emerging in the "cold fusion" field. One of the most disputed anomalies in the history of science is inexorably heading toward acceptance by the scientific community.

In May 1991, when I published my account of the cold fusion quest to that point, Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor (John Wiley & Sons), I suggested that the evidence for "cold fusion" phenomena was overwhelmingly compelling. I acknowledged then that there was a remote chance that it could all be wrong, and I was not certain that technological applications would emerge quickly. But each year since 1991, the evidence has grown more convincing. Now I believe that the case for a spectacular new class of phenomena is 100-percent proved, though I am not sure which of the dozen or so theories best explains the expanding category of anomalous nuclear and nuclear energy-magnitude phenomena. That there will be technological applications is now certain, whether or not the phenomenon is completely understood by that time.

It has been over five years since the March 23, 1989 announcement by Drs. Fleischmann and Pons at the University of Utah. Now it is abundantly clear that their widely denied discovery was but the "tip of an iceberg," in which the boundary between chemistry and nuclear physics would be blurred forever. "Cold fusion" may or may not be literally the "fusion" of hydrogen isotopes at room temperature, but whatever the detailed microphysical explanation turns out to be, it will, in part, involve nuclear reactions. It will also have to incorporate the release of macroscopic energy at prodigious levels that are consistent with nearly radiationless nuclear reactions.

You may think that alchemy has risen from the grave. Indeed, in some sense it has. The transmutation of heavy elements in presumptively "chemical" systems has also been observed in a variety of cold fusion experiments. In fact, one researcher at a well-known university in the U.S., who has been highly skeptical of cold fusion, nonetheless performed a Fleischmann-Pons-type experiment and serendipitously discovered unambiguous gamma ray evidence of no less than seven radioactive isotopes that were formed in his palladium cathodes—isotopes of rhodium, silver, and ruthenium. (In 1993 and 1994 this researcher chose not to publish his results, so as not to be ridiculed by his physicist colleagues, but we are confident that he will publish eventually.) The implications for other areas of science: astrophysics, chemistry, geology, and possibly even biology—— may very well be profound.

The technological and social implications of "cold fusion" are as astonishing as the scientific ones. It means nothing less than the end of the fossil-fuel age, the demise of fission nuclear power, and the beginning of a water-fueled—a hydrogen isotope-fueled—civilization. Energy will be cheap and environmentally benign. The fuel reserves are effectively infinite and available to all nations. So please do not begrudge us that "logically impossible" title, INFINITE ENERGY. What if the claimed over-unity "weird machines"—magnetic motors, Hydrosonic Pumps, underwater plasma-arc discharges (shock-induced excess power), and the like are also really producing more power out than in? Will anomalous nuclear reactions be found to explain them? Or will the energy be coming from the tapping of Zero Point Energy (ZPE)—the quantum fluctuations in space-time itself? We'll leave those issues to the theorists, whom we welcome to our pages as well.

Prototype cold fusion systems that can heat homes and produce electricity are expected to emerge within a year. The pre-prototypes of these devices are actually here already. With the advent of these robust devices, the scientific and technological rev-

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olution will accelerate dramatically. Skeptics who felt comfortable attacking alleged "subtle mistakes" in cold fusion experiments will be hard-pressed to deny these practical devices. Money talks. When the largest industrial corporations in the world rush belatedly into cold fusion R&D—as some already have done, particularly in Japan, but increasingly in the United States—the controversy will be over.

To the uninitiated, these conclusions admittedly may seem rash. However, the evidence for the anomaly of excess energy production at levels that cannot be explained by chemical reactions has not gone away. In fact, it has appeared in an increasing variety of different chemical and physical systems. Nor have the numerous by-products of nuclear reactions occurring near room temperature gone away: heretofore, these were thought to be totally impossible. In fact, with each passing month these lines of evidence get stronger as they are revealed with a multiplicity of experimental techniques. These impressive results are increasingly appearing in peer-reviewed journals, such as Physics Letters A, the Journal of Electroanalytical Chemistry, the Japanese Journal of Applied Physics, and Fusion Technology, to cite but a few examples. Unfortunately, Nature and Science still ignore cold fusion, but soon they too will see the light. Perhaps it is fortuitous that the present editorial leadership of both Science and Nature magazines is now in transition. The new editors of those magazines will be well-advised not to employ the unrepentant, knee-jerk negativism toward cold fusion practiced by Drs. John Maddox at Nature and Daniel Koshland at Science.

There have been recent media breakthroughs. Popular Science ran a positive cover story on cold fusion in August 1993. The May/June 1994 issue of MIT Technology Review featured a cold fusion cover story by cold fusion researcher Dr. Edmund Storms, who recently retired from Los Alamos National Laboratory. The British Broadcasting Corporation and the Canadian Broadcasting Corporation co-produced a highly positive hour-long television documentary, "Too Close to the Sun," aired in the spring of 1994. Professor Keith Johnson of MIT, a cold fusion theorist, has even written the techno-thriller script for a forthcoming Hollywood movie with a cold fusion theme —Excess Heat.

"Cold Fusion" Magazine, the first three issues of which I edited, was launched in 1994. Then it became a newsletter with very uncertain direction. Unfortunately, this originally well-received pioneering journal was published in an unsatisfactory environment, and all the editors found it necessary to continue their pursuit of cold fusion elsewhere. Now they have a new home, INFINITE ENERGY: Cold Fusion and New Energy Technology. We are proud of our logo, the infinite surface Mobius strip, which in profile happens to look like the symbol for infinity.

If you are new to the field, please avail yourself of our resources. First, you should obviously become subscribers to our presently bi-monthly publication. Next, you should consider purchasing some of the reference material offered in this publication, particularly the set of

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Fusion Facts,

a monthly scientific newsletter covering worldwide research in the cold fusion phenomena.
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Fusion Information Center
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Also available, *Cold Fusion Impact in the Enhanced Energy Age*, a book about the near future of the world energy situation. Phone for information: 801-583-6232

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four video tapes of MIT Cold Fusion Day 1995— eight (8) hours of provocative material. Much food for thought, we promise you!

For those who want a quick update of what has gone before in the cold fusion field, you should consider acquiring (for only $5.00) our Cold Fusion/New Energy Technology Resource Guide, which was the precursor to this magazine. The editors are currently working on a much more extensive Resource Guide, but that won't be published 'till much later in 1995.

Last, but definitely not least, we welcome aboard Hal Fox of the Fusion Information Center in Salt Lake City as our Associate Editor. Since 1989, Hal has been a true pioneer in cold fusion and new energy technology with his highly influential newsletters, Fusion Facts and, later, New Energy News. We urge you to subscribe to these also, even as our two organizations increasingly share information and editorial material.

Eugene F. Mallove, Sc.D.
Editor-in-Chief.

Frontier Perspectives

The history of science has shown repeatedly that novel scientific discoveries now believed to be true were typically disregarded in their time. Unfortunately, the present is no exception; frontier scientists who dare to work at the edges of science face extraordinary obstacles. The Center for Frontier Sciences was establishes in 1987 to coordinate globally information exchange, networking, and education on frontier issues of science, medicine, and technology. The Center helps engender greater openness to novel scientific claims in several areas, including bioelectromagnetics, the mind-matter interrelationship, complementary medicine, and new energy technology.

If you wish to receive Frontier Perspectives regularly, 2 issues per year, as well as notification of events hosted by the Center, the annual subscription fee is $30.00.

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Letters to the Editor

I just received your new publication. To steal a phrase, "Jack is Back!"

Thanks for your perseverance. It's great to rediscover how alive and well the "cold fusion" community still is. This is real adventure, real science. Maybe the excitement and controversy surrounding this field is, in some ways similar to the wonderfully stimulating atmosphere in physics during the 20's and 30's, as the giants of that age began to unravel the mysteries of atomic structure ... and atomic power.

I'll be pleased to see my essay in print. While it may not soften the stone-heads who continue to insist that theory supersedes experimental evidence, it might motivate other, more open-minded folks to jump into the fray.

At the risk of being much too politically incorrect, I believe there is nothing wrong with our modern energy-intensive American life-style that unlimited, non-polluting energy can't fix. Then again, didn't Gandhi warn his countrymen of the spiritual dangers of intemperate materialism? But even those who would lead a more simple life will benefit from new hydrogen energy and its ability to provide energy economically on a decentralized basis. The frontier spirit may live again ... yet with many of essential blessings of technology, such as refrigeration, heating, and lighting.

Geoff Rohde
New Venture Marketing
Davis, CA

Yesterday I received the first issue of your periodical, *CF/NET*. Well, cold fusion seems to be a very optimistic chance for mankind... Let's hope, the big oil companies and other multinationals won't buy it away, or worse... It is time for a big and rapid change in our society, don't you agree? Time to make that society we all know of in our dreams, time to develop a clean society.... But it will cost a lot of jobs... I am just a very interested layman, wishing a better, cleaner future for mankind and I think this project is a very hopeful development.

Is there in Europe, or especially here in Holland, a research group or group of interested persons with special attention for Cold Fusion? If there is, is it possible for you to have them contact me?

Johan P.M. Roland,
Purmerend, The Netherlands

I presented an invited paper, "Cold Fusion: Current Status and Future Prospects," at the 53rd Annual Meeting of the Oregon Academy of Science, held at Reed College in Portland on 2-25-95. At the end of my talk I distributed copies of your fax. Two of my graduate students talked about their cold fusion research at the same meeting.The questions from the audience [about 20] indicated curiosity. There was no open hostility.

Enclosed is a paper [*Fusion Technology* 1993] which describes the system which we study. We use a thin, cold rolled Pd cathode, a parallel Pt anode, an electrolyte containing D_20 and H_2SO_4, and current densities in the range 0.5 to 2 A per sq cm. Reactions are fast and reproducible. This paper gives results for a 12 min. experiment, including surface pits which are particularly well characterized by means of an atomic force microscope. We believe that localized melting and vaporization of Pd occurs, since Pd is found in the electrolyte after the experiment. If true, there is no explanation based on any known chemical reaction, because the heat required is about 100 times more than a chemical reaction could deliver.

Also enclosed are three additional publications. These show evidence for excess heat which could be explained by localized concentrations of unexpected elements. Our system behaves as though low energy neutrons are produced. Although we have tried, we have never detected radiation outside our cells. Our results also indicate that substitution of an electrolyte containing H_20 and H_2S0_4 gives the same type of results but to a lesser degree.

Our work is continuing. Five highly motivated graduate students are currently involved in this research.

Thank you for your interest, and best regards.

John Dash, Professor

College of Liberal Arts and Sciences,
Department of Physics
Portland State University
Portland, Oregon

Dear Dr. Mallove:

Your interesting and informative letter to the editor of *Physics Today* (March, 1994) suggests to me that you may be interested in the enclosed copy of my manuscript, "D_2-charged Pd electrodes' anomalous energy productions: induced beta decays?"

Data from sonoluminescence experiments and from Pd/D_2O electrolytic cells

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should of course continue to be analyzed in terms of nuclear fusion as long as that process in fact has not been foreclosed. But peculiarities of some data from the latter indicate an absence of nuclear fusion; beta decay then is a natural alternative hypothesis. My manuscript offers a process by which beta decay might be induced.

I have submitted the manuscript to a major chemistry journal, along with a rationale for publishing a "physics" article therein.

Robert B. Driscoll
Oakland, California

[Robert Driscoll subsequently informed us that his article *has* been accepted for publication and will appear in a few months. — EM]

Dear Gene,

A belated Merry Christmas, and welcome to 1995, which I think might be the turning point year. Also I think I owe you an apology because I have until recently been a bit negative about cavitation light water fusion. It does seem pretty wild to think of getting heat out of ordinary water with relatively simple devices. As you probably know, I went to Russia in September and had a chance to get to know some of their physicists, and maybe more to the point, to get some feeling for the science climate and people's interrelationships. I went with a colleague, who is an ex- U.S. Air Force officer and managed a nuclear program for some years. The meeting was at Sochi on the Black Sea, and it was a group of some 40 mostly theoretical physicists, who to my view approached the cold fusion problem a bit more quantum mechanically than Drs. Hagelstein and Preparata.

Though to my view the Russians were still off the mark, except for Alexander Krivoshein, who talked in the language of quantum field theory, which is the basis for band theory. One didn't get much of a feeling for their experimental work, except for one man who had been working the molten salt problem. What impressed me was that he did not try to hide the marginal nature of his results. My general feeling was that the scientists reported what they saw. Reiko Natoya was there and gave a good talk on light water electrochemical "fusion."

But what did happen that is exciting was that a colleague returned to Moscow with his family about three weeks ago and saw the Energiya "thermal reactor" in operation. He was shown the thermal reactor by one of the theoretical physicists and by an ex-Russian Air Force General, who had been in the space program. The family was taken to Star City and shown the Russian Shuttle or Space Station, and the boys got to sit in the astronaut seats. But returning to the "thermal reactor." It is a "hydrosonic pump"-type device that apparently runs with ordinary water. It is designed to produce cavitation and also to have long life. The reactor was driven by an electric motor pump combination and circulated closed-loop water through some sort of labyrinth which caused turbulence and cavitation. They turned it on for my friend, and it quickly heated up to 60°C or so. They said it was outputting 12 kW of heat. He could only say it got hot quickly. Their output heat numbers could be roughly calculated from how fast the device heated up, using the heat capacity of 20 liters of H2O and the mass of enclosing metal. How can they be wrong? My general impression is that they are honest and unlikely to do anything to fool my colleague.

All of the above, plus an article in the 21 Nov *Phys. Rev. Lett.* on cavitation led me to reconsider the cavitation evidence. I looked for issue 2 of *Cold Fusion* and couldn't find it at first, but I did find the Griggs article in the ICCF4 *Proceedings.* This time I read it pretty carefully. Again the calorimetry seemed straightforward, with its use of 55 gallon drums of water to condense steam and observe the temperature rise. Griggs also talked about your and Rothwell's visit. Later I found *Cold Fusion* 2 and read your article carefully.

Getting heat from ordinary water seems a bit improbable, except when you think of the conditions existing during cavitation: 50 atm of pressure for 10^{-9} s, water-compression-heating raising the water temperature to 120°C, with some of the water crystallizing to ice VII. Lots of shock wave excitation to put all kinds of ions into excited states, which with a solid, and maybe even with a liquid could mean band state occupations. The point is that the ion band state configuration seems to overcome the coulomb barrier even in quite small crystals. One possibility is an impurity-deuterium reaction like $D + -D^+$, or maybe more likely, the $H^+ + D^+ {}^3He^{++}$ reaction. Another possibility could be $H^+ + {}^{18}0^+ \; {}^{19}F^{++}$, or maybe $H+ + {}^{18}0^- \; {}^{19}F$, which would work with no deuterium, but raises the question as to whether the electrons around $^{18}0$ would prevent overlap. I don't think so, but Scott is more questioning. I remain convinced that if one gets excess heat, it has to be nuclear, and if one gets excess heat without radiation, it has to be the ion band state process or its equivalent. (The Russian theorist doesn't think their thermal reactor runs on cold fusion.)

The climate for cold fusion research is not good in most US locations as far as I can see, but the evidence gets better. Weird! So I guess we face another difficult year. But I still think success is just around the corner.

Good luck for the New Year.

Talbot A. Chubb
Arlington, Virginia

The enclosed check is to have you send the *CFNET* update to all of us. Amazing stuff! Let them know I had it sent, so it won't get immediately trashed!

Ed Wall
U.S. Department of Transportation,
Federal Aviation Agency

I have been following the development of cold fusion through your interviews on the Laura Lee Show in Seattle. Can't wait to subscribe to your new cold fusion magazine. Please send me information on how to subscribe as soon as you are ready to publish. I am sure cold fusion would be a dead issue if it were not for people like you. Thank you for for hanging in there and keeping the truth flowing.

Judy Leary
Suquamish, Washington

Dear Dr. Mallove:

Before he left for school this morning, Tim asked me to check CompuServe later to see if you had left your address. Having found that you did, I am sending this in his behalf.

Please accept my heartfelt appreciation for the support you have provided him in his effort to inform himself about cold fusion. I am sure you share my feeling that young people with ability must be encouraged to optimize their talents if we, as a nation, are to provide our population in the future with the advantages we have known in the past. I can tell you that Tim is bubbling with enthusiasm over the subject he has chosen for his essay and it is largely because of the help you have given him and the interest you have displayed in his work.

He will certainly let you know if his efforts result in recognition by the judges of the duPont competition.

J. Timothy O'Leary, D. D. S.
Girard, Pennsylvania

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Enclosed find my personal check for a copy of the *CFNET* Update. I am also enclosing my Request to be Placed on Mailing List, for continuing information on your efforts to keep Cold Fusion and New Energy Technology in the public forum. Best wishes for every success on your plans for a new cold fusion magazine.

I have given two well-received seminars on Cold Fusion to the Science and Engineering faculty at Bakersfield College, and stirred up considerable general interest. With that interest in mind, I plan to re-present updated versions of these seminars to the general campus community, both faculty and students, this spring.

Keep up your efforts! This is a field whose time has come, and I congratulate you for being on the crest of the wave, instead of being one of so many who will merely be overwhelmed by it. Thank you.

Richard E. Wise
Associate Professor, Life Science/Physical Science
Bakersfield College, CA

Hello Eugene Mallove:

I was listening to National Public Radio last year and heard a most interesting show on cold fusion. You were the guest speaker. I finally received information from NPR about an address at which I could reach you. Would you please inform me as to addresses at which I can subscribe to several of the cold fusion magazines and journals you mentioned in your talk? I am most interested in the topic.

Thanks so much for your help.

Anne Lonsky
Auburn, New York

Many thanks for the copy of your fine newsletter, and for your interest in my book [*Dark Matter, Missing Planets, & New Comets: Paradoxes Resolved, Origins Illuminated*, North Atlantic Books, Berkeley, CA, 1993]. I enclose a copy of the *Meta Research Bulletin*, which I edit, in case you might have some interest therein.

You asked about the excess energy (I assume you mean heat flow) from the outer planets. In my opinion, the most probable explanation is gravitational compression, the traditional heat source for planets. In the conventional wisdom, an equilibrium is supposed to be reached after some millions of years. But as you will see from my book, I have collected extensive evidence for the exploded planet hypothesis, which nominally upset that equilibrium just 3.2 million years ago. If the hypothesis is correct, then we expect present-day excess heat flows from all the planets more or less in proportion to their masses, roughly as is seen.

Tom Van Flandern
Meta Research, Washington, D.C.
(Former Director of the Celestial Mechanics Branch of the Nautical Almanac Office of the U.S. Naval Observatory)

It was interesting to hear how Caltech's David Goodstein trashed cold fusion on National Public Radio the day before the MIT cold fusion IAP meeting. He did the same thing regarding high-temperature superconductors a year or two ago in a Caltech public lecture I went to. His main objection to HTS's was that they couldn't handle large currents, and yet HTS's are brand new and it took decades for low-temperature superconductors to get to where they are now.

I struck out with JPL and Caltech's libraries — neither has *Fusion Technology*. May I trouble you to e-mail me the address of the American Nuclear Society so I can have my own copy of the December 1994 issue?

Stephen F. Brown
MIT Class of 1965

I enjoyed your good service as editor of "Cold Fusion", and was sorry to see you leave. With the fourth, reduced issue of "Cold Fusion" it was immediately obvious to me that Wayne Green is a terrible editor andcomplete incompetent for this CF subject matter. So, after paying $100for a "Cold Fusion" subscription, I am now stuck with the remainder of that subscription being fulfilled with product I consider worthless.

Best wishes on your new venture. I hope it succeeds for you, and I look forward to reading your magazine (Tinsley was also good, and I hope to see him in your new mag).

Kurt Johmann
Gainesville, FL

Enclosed please find two checks, one for the set of videotapes you so graciously sent on a telephone order and the other for the *CFNET*.

I have enclosed two photographs of Michael's home "laboratory." He is continuing to work on the light water reactions for science fair projects and his own personal exploration. He is learning more about conducting good scientific research and is planning on pursuing a field of alternative energy research. He would like to set up his own laboratory/experiment station some day.

Again, thank you for sending the videotapes so promptly. We would be most pleased to have Michael's work published.

Michael Belcher
Princeton, West Virginia

March 20, 1995

Dr. Eugene F. Mallove
COLD FUSION ENTHuslAsT
171 Woodhill-Hooksett Road
Bow, NH 03304

Happy first day of spring!

Was just getting around to perusing the January issue of CFNET, and without being hypercritical I noticed a time-honored inaccuracy in *Highlights of Cold Fusion History: A Time-Line"* on p. 26, with respect to Kamerlingh Onnes. I've been working on a set of biographical vignettes entitled "Scientists who were Shafted," which attempts to set the record straight historically and which you might find interesting—

"The history of superconductivity is intimately connected with that of cryogenics. That history began 115 years ago, in 1877, with the first liquefaction of oxygen by the French physicist Louis Paul Cailletet and the Swiss Raoul Pierre Pictet. By 1894 the Dutch physicist Heike Kamerlingh Onnes demonstrated the first commercial liquid-air plant, and four years later the British chemist Sir James Dewar liquefied hydrogen. But it wasn't until 1908 that Onnes succeeded in liquefying helium, the most difficult of all the gases to liquefy because of its extremely low 4.2° K (-269° C) boiling point. Three years later superconductivity was discovered in a rod of frozen mercury by Georg Holst, a student of Onnes, who was assisted by G.J. Flim, an instrument maker and master craftsman of Leiden who, himself, had built the first helium liquefier. Flim later founded a school for instrument makers, while Hoist became the first director of the Philips Research Labs.

"Onnes, himself, was highly skeptical that a metal such as mercury could just suddenly lose all resistivity to an electric current, and didn't believe Holst's finding until he had repeated the experiment several times himself. That same year (1911), Onnes published the results of those landmark experiments as an appendix to another paper, in which he did give credit

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to Hoist but none to Flim. Two years later Onnes received the Nobel award for his work in cryogenics. And, although one might be tempted to say that he wasn't the principal discoverer of record for superconductivity, which indeed he was not, the accomplishments of Onnes were nevertheless impressive."

Raptures of the Deep was finally published by Cahners a few weeks ago, and except for some minor boo-boos looks pretty good. They flew me to New Orieans for its promotion, and that's about the only travel I really want to do this year. So don't look for me in Monaco, although it's tempting.

Keep the faith, *mon vieux*.

Ciao, for now,
Frederic Jueneman
Newark, California

"Let us hope that in a decade or two, or, at least, just before the beginning of the twenty-first century, the present meager years of theoretical physics will come to an end in a burst of entirely new revolutionary ideas similar to those which heralded the beginning of the twentieth century."

George Gamow
Russian-American
Physicist
1904-1968

(Quote, courtesy,
Evan Ragland)

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Julian Schwinger—A Fond Remembrance by Eugene Mallove

Julian Schwinger, whose contributions to quantum physics are legendary, lived to see the difficult birth of the Cold Fusion Age.

Born in 1918 in New York City, in his youth was a boy genius with a rebellious streak. That almost prevented him from receiving his doctorate from Columbia University. Beginning in 1945 he taught at Harvard University and in 1972 he joined the faculty of the University of California, Los Angeles, where he was a distinguished professor of physics.

His contributions to quantum electrodynamics eventually led him to share the Nobel prize in 1965 with Richard P. Feynman and Shin'ichiro Tomonaga, both of whom he outlived.

On July 16, 1994, he died after the sudden onset of an illness, working to the very end on physical theories, including his theory of sonoluminescence—which he had metaphorically connected with cold fusion on several occasions. He left behind his wife of 47 years, Clarice Carrol Schwinger, as well as a host of compatriots in cold fusion research. By coincidence, he died on the precise day of the 25th anniversary of a pioneering event: the launching of the first manned expedition to the lunar surface —Apollo 11.

For Julian's great contributions to the cold fusion field—both in theoretical support and in his solidarity with fellow scientists — we can do no less than reprint here one of his great works. We lead with his erudite talk to the Fourth International Conference on Cold Fusion, held in Maui, Hawaii in early December 1993.

I first met Julian Schwinger and his wife Clarice on a memorable evening in Salt Lake City, in March 1990 at the First Annual Conference on Cold Fusion. I next had direct contact with him when in the spring of 1991 while still at MIT as chief science writer at the MIT News Office, I received a welcome notice from John Wiley & Sons, my publisher. The excited voice over the telephone said that Schwinger had read the draft manuscript of my cold fusion book, *Fire from Ice*, and had praised it with characteristic elegance thus, "Eugene Mallove has produced a sorely needed, accessible overview of the cold fusion muddle. By sweeping away stubbornly held preconceptions, he bares the truth implicit in a provocative variety of experiments." I can't deny that my head spun upon receiving that praise from a Nobel laureate.

Thus began a long friendship through what might be called the first five years of the "Cold Fusion War." I soon learned that Julian had resigned from the American Physical Society rather than put up with some of its members' wanton censorship of his theories about cold fusion. He would subsequently publish his ideas in other journals, and in the Proceedings of the National Academy of Sciences.

Many a time since 1991 I would call Julian late at night (in my eastern U.S. time zone) and Clarice would tear him away from his work so that we could chat — often for an hour—about the latest developments in experiments, theories, and politics. I am proud to say that Julian Schwinger signed a petition to the Congress of the United States—in particular to the House Science, Space, and Technology Committee—urging a Congressional reassessment of research funding for cold fusion. Unfortunately, the Congress did nothing to move the immovable objects at DOE, who lavish funding on hot fusion and ignore cold fusion.

It is impossible to write enough in this short space that would honor Julian to the extent that he deserves. It is best simply to provide below a record of his published contributions to the related fields of cold fusion and sonoluminescence. The cover story of this premier issue speaks of a deep connection between the two enigmatic phenomena.

Schwinger had the deep respect of other theorists, even as they disagreed with some of this ideas. This is what MIT cold fusion theorist Peter L. Hagelstein said of Julian in his tribute in *Fusion Technology* (December 1994): "He was one of my heroes. Even though I did not know him personally, I feel a personal loss as well as a loss to the world of one of the greatest minds of our time."

In one of his cold fusion essays, Schwinger issued a stern warning that is still being ignored by the denizens of the scientific establishment: "The pressure for conformity is enormous. I have experienced it in editors' rejection of submitted papers, based on venomous criticism of anonymous referees. The replacement of impartial reviewing by censorship will be the death of science." One of those denizens admits in his obituary on Schwinger that he refused to read what Julian had to say about cold fusion." Physicist-author Jeremy Bernstein, writing in the Spring 1995 *American Scholar*: "The last news I had about Julian, before his unexpected death in July, was that he had become interested in cold fusion. Someone sent me a reprint of a lecture that Julian had given in Japan on the subject. I didn't have the heart to read it."

Julian's spirit lives on and will have the last laugh—that's for sure.

- "Nuclear Energy in an Atomic Lattice." *Proceedings of the First Annual Conference on Cold Fusion*, March 28-31, 1990, Salt Lake City, pp. 130-136.
- "Cold Fusion: A Hypothesis," *Zeitschrift Fur Naturforschung* Vol. 45, No. 5, May, 1990, p. 756.
- "Cold Fusion: Does it Have a Future?" in *Evolutional Trends of Physical Sciences*, Springer Verlag, 1991. (From a talk delivered in Tokyo, 1990)
- "Phonon Representations," Proc. Natl. Acad. Sci., Vow 87, September 1990, pp. 6983-6984.
- "Phonon Dynamics" *Proc. Natl. Acad. Sci.*, Vol. 87, November 1990, pp. 8370-8372.
- "Nuclear Energy in an Atomic Lattice—Causal Order", *Prog. Theor. Phys.*, Vol. 85, No. 4, April 1991, pp. 711-712.
- "A Progress Report: Energy Transfer in Cold Fusion and Sonoluminescence," a lecture delivered at MIT and at the University of Pennsylvania, autumn 1991.
- "Casimir Energy for Dielectrics," *Proceedings of the National Academy of Sciences*, Vol. 89, May 1992, pp. 4091-4093.
- "Casimir Energy for Dielectrics: Spherical Geometry," *Proceedings of the National Academy of Sciences*, Vol. 89, December 1992, pp.l 118-1120.
- "Casimir Light. Pieces of the Action," *Proceedings of the National Academy of Sciences*, submitted, 1993.
- "Cold Fusion Theory: A Brief History of Mine," a talk for the Fourth International Conference on Cold Fusion, Maui December 6-9,1993; in *Fusion Technology*, Vol.26, December 1994.

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Nobel laureate Julian Schwinger's talk at the Fourth International Conference on Cold Fusion, ICCF4, Maui, Hawaii, December 1994. Because Julian was not able to attend ICCF4, his presentation was read to an evening session by Eugene Mallove. This was the second talk that Schwinger had delivered to a cold fusion conference, the first having been in Salt Lake City in March 1990 at the First Annual Conference on Cold Fusion. —Ed.

Cold Fusion Theory:

A Brief History of Mine

As Polonious might have said: "Neither a true-believer nor a disbeliever be." From the very beginning in a radio broadcast on the evening of March 23, 1989, I have asked myself not whether Pons and Fleischman are right—but whether a mechanism can be identified that will produce nuclear energy by manipulations at the atomic—the chemical—level. Of course, the acceptance of that interpretation of their data is needed as a working hypothesis, in order to have quantitative tests of proposed mechanisms.

As a long-time nuclear physicist, the knee jerk reaction to the idea of a D-D reaction without significant neutron production brought in words like ^{4}He and Mössbauer effect. I tried, without success, to contact P(ons) and F(leischman), to the point of sending a letter to the Los Angeles Times, which was garbled in the editing process. Finally, with the help of a friend, contact was made in the early part of April and I went to Salt Lake City.

There, I was assured that they knew about ^{4}He, and was shown a peak in a spectroscopic read-out which, I was told, was ^{4}He. Soon after my return to Los Angeles, references to ^{4}He disappeared, to resurface only relatively recently.

I do not have to-but shall-remind you of the two fundamental problems that the acceptance, of P & F's excess heat as nuclear in origin, entails.

1. What accounts for the absence of particles that are familiar in ordinary hot fusion, such as the neutrons of D + D→ n + ^{3}He and the high energy γ-ray of D + D → γ + ^{4}He? Very early in my thinking I added the conventional reaction p + D → γ + ^{3}He. Why? Mostly because it would also be there. One cannot produce heavy water without some contamination by light water.

2. Hot fusion relies on achieving enough kinetic energy to overcome the Coulomb repulsion between like charges. How then can cold fusion, operating far below those levels, ever achieve fusion? Incidentally, I have read, and heard, that my solution to the Coulomb barrier problem is to forget it! Not even an absent-minded professor (which I am not) would go that far. Critics should learn to operate within the bounds of sanity.

My first attempt at publication, for the record, was a total disaster. "Cold Fusion: A Hypothesis" was written to suggest several critical experiments, which is the function of hypothesis. The masked reviewers, to a person, ignored that, and complained that I had not proved the underlying assumptions. Has the knowledge that physics is an experimental science been totally lost?

The paper was submitted, in August 1989, to Physical Review Letters. I anticipated that PRL would have some difficulty with what had become a very controversial subject, but I felt an obligation to give them the first chance. What I had not expected—as I wrote in my subsequent letter of resignation from the American Physical Society—was contempt.

"Hypothesis" was eventually published, after protracted delays, in a 1990 issue of a German periodical. Does it have any significance in 1993? I cite the following excerpts: "... this cold fusion process (of P & F) is not powered by a DD reaction. Rather it is an HD reaction, which feeds on the small contamination of D_20 by H_20.", and:

"The HD reaction p + d → ^{3}He does not have an accompanying g-ray; the excess energy is taken up by the metallic lattice of Pd alloyed with D."

and finally:

"... concerning the oft repeated demand for a control experiment using H_20, one should note the possibility of a converse effect of the HD reaction: Through the natural presence of D_20 in ordinary water, such control experiments might produce an otherwise puzzling amount of heat."

A following paper, entitled: "Nuclear energy in an atomic lattice, 1," was sent directly to another German periodical, in November of 1989. As of today, the only memorable part is a quotation from Joseph Priestly:

"In this business, more is owed to what we call chance–that is, to the observation of events arising from unknown causes-than to any preconceived theory."

The editor thought it necessary to add a total disclaimer of responsibility, ending with: We leave the final judgement to our readers." In my naivity I had thought that was always so. When part 2 of NEAL was submitted, it was simply rejected. The fix was in.

I gave a talk with the same title—"Nuclear Energy in an Atomic Lattice"—at Salt Lake City in March of 1990. The HD hypothesis—of the dominance of the pd reaction—has the pragmatic advantage of suppressing neutron production at the level of excess heat generation.

But, to quote from that lecture: "... a well trained hot fusioneer will instantly object that there must also be a 5.5 MeV γ-ray. He will not fail to point out that no such radiation has been observed. Indeed."

"But consider the circumstances of cold fusion. (The quotation continues.) At very low energies of relative motion, the proton and deuteron of the HD reaction are in an s-state, one of zero orbital angular momentum, and therefore of positive orbital parity. The intrinsic parities of proton, deuteron, and 3He are also positive. Then, the usually dominant electric dipole radiation–which requires a parity change–is forbidden."

I turn from 'missing' radiation to Coulomb repulsion, and quote:

"... treatments of nuclear fusion between positively charged particles (usually) represent the reaction rate as the product of

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two factors. The first factor is a barrier penetration probability. It refers entirely to the electric forces of repulsion. The second factor is an intrinsic nuclear reaction rate. It refers entirely to nuclear forces. This representation ... may be true enough under the circumstances of hot fusion. But, in very low energy cold fusion one deals essentially with a single state, or wave function, all parts of which are coherent. It is not possible to totally isolate the effect of the electric forces from that of the nuclear forces: The correct treatment of cold fusion will be free of the collision-dominated mentality of the hot fusioneers."

To speak of transferring energy to the lattice is to invoke lattice excitations, or phonons. At about the time of the Salt Lake City meeting, or shortly after, I became dissatisfied with my treatment, and began to reconstruct phonon theory. A note entitled "Phonon representations" was submitted to the Proceedings of the National Academy of Sciences in June of 1990. The abstract reads:

"The gap between the nonlocalized lattice phonon description and the localized Einstein oscillator treatment is filled by transforming the phonon Hamiltonian back to particle variables. The particle-coordinate, normalized wave function for the phonon vacuum state is exhibited."

A month later, I submitted a second note with the title "Phonon dynamics." The abstract reads:

"An atomic lattice in its ground state is excited by the rapid displacement and relase of an atomic constituent. The time dependence of the energy transfer to other constituents is studied ...."

The third and last note is called "Phonon Green's function." Its abstract is:

"The concepts of source and quantum action principle are used to produce the phonon Green's function appropriate for an initial phonon vacuum state. An application to the Mössbauer effect is presented."

I remind you that the Mössbauer effect refers to "an excited nucleus of an atom, imbedded in a lattice, (that) decays with the emission of a g-ray," thereby transferring momentum to the lattice. "There is a certain probability ... that the phonon spectrum of the lattice will remain unexcited, as evidenced by the absence, in the g-ray energy, of the red-shift associated with recoil energy."

A casual explanation of the Mössbauer effect has it that the recoil momentum is transferred to the lattice as a whole so that the recoil energy, varying inversely with the mass of the entire lattice, is extravagantly small. As Pauli would say, even to God, "Das ist falsch!" The spontaneous decay of a single excited atom in the lattice is a localized event, the consequences of which flow at finite speed, out into three dimensional space, weakening as they travel. This is a microscopic event, with no dependence on macroscopic parameters such as the total mass of the lattice.

Unmentioned in the abstract, but of far greater importance, is another situation. To quote: "What happens if the momentum impulse ... is applied, not to one, but all lattice sites?" The reader is invited to "recall that the lattice geometry is not absolute, but relative to the position of the center of mass for the entire system. Thus the injected energy can be read as the kinetic energy transferred to the lattice as a whole." More of this shortly.

In the last month of 1990, I went to Tokyo. The occasion was the 100th anniversary of the birth of a famous Japanese physicist, perhaps most familiar for his part in the Klein-Nishima formula for Compton scattering. On a day that, to my surprise, I found uncomfortably close to another infamous day, I delivered a lecture on: "Cold Fusion Does It Have a Future?" The abstract reads:

"The case against the reality of cold fusion is outlined. It is based on preconceptions inherited from experience with hot fusion. That cold fusion refers to a different regime is emphasized. The new regime is characterized by intermittency in the production of excess heat, tritium, and neutrons. A scenario is sketched, based on the hypothesis that small segments of the lattice can absorb relased nucelar energy."

I pick up the last sentence of the abstract with this quotation from the text:

"If the γ-rays demanded by the hot fusioneers are greatly suppressed, what agency does carry off the excess energy in the various reactions? One must look for something that is characteristic of cold fusion, something that does not exist in the plasma regime of hot fusion. The obvious answer is: the lattice in which the deuterium is confined.

Imagine then, that a small, but macroscopic piece of the lattice absorbs the excess energy of the HD or DD reaction.... I advance the idea of the lattice playing a vital role as a hypothesis.... Intermittency is the hallmark of cold fusion.... Does the lattice hypothesis have a natural explanation for intermittency? ... a close approach to saturation loading is required for effective fusion to take place. But, surely, the loading of deuterium into the palladium lattice does not occur with perfect spatial uniformity. There are fluctuations. It may happen that a microscopically large—if macroscopically small—region attains a state of such lattice uniformity that it can function collectively in absorbing the excess nuclear energy that is released in an act of fusion. And that energy can initiate a chain reaction as the vibrations of the excited ions bring them into closer proximity. So begins a burst. In the course of time, the increasing number of vacancies in the lattice will bring about a shut-down of the burst. The start-up of the next burst is an independent affair. (This picture is not inconsistent with the observation of extensive cracking after long runs.)

What answer did I give, just three years ago to "Does cold fusion have a future?" I said: "I have little hope for it in Europe and the United States–the West. It is to the East, and, specifically, to Japan that I turn."

Inspired by good soba and sushi, I dashed off a short addendum that Progress of Theoretical Physics received in January and published in April of 1991. The abstract of "Nuclear Energy in an Atomic Lattice-Causal Order" is:

"The extremely small penetrability of the Coulomb barrier is generally adduced to dismiss the possibility of low energy (cold) fusion. The existence of other mechanisms that could invalidate this logic is pointed out."

Here are excerpts. "... Implicit in this line of thought (of negligible penetrability) is the apparently self-evident causality assignment that has the release into the surrounding environment, of energy at the

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nuclear level, occur after the penetration of the Coulomb barrier. One would hardly question that time sequence when the environment is the vacuum. But does it necessarily apply to the surrounding ionic lattice? ... another reading is possible, one in which the causal order is reversed. Why? Because, in contrast with the vacuum, the lattice is a dynamical system, capable of storing and exchanging energy.

The initial stage of the new mechanism can be describd as an energy fluctuation, within the uniform lattice segment, that takes energy at the nuclear level from a pd or dd pair and transfers it to the rest of the lattice, leaving the pair in a virtual state of negative energy....

For the final stage ... consider the pd example where there is a stable bound state: ^{3}He. If the energy of the virtual state nearly coincides with that of 3He, a resonant situation exists, leading to amplification, rather than Coulomb barrier suppression.

It would seem that two mechanisms are available .... But are they not extreme examples of mechanisms that in general possess no particular causal order?"

The last lecture on cold fusion was delivered—twice—in the Fall of 1991, to celebrate the birthdays of former students, one of whom is at MIT, a hotbed of hot fusioneers. The cover title: "A Progress Report," injects a bit of my own nostalgia. Not long after the simultaneous arrival of myself at Berkeley and World War II, Robert Oppenheimer gave a lecture with that title. As he explained, it meant only that time had elapsed. That also applied to the first part of my birthday lectures—"Energy Transfer in Cold Fusion"—with one exception:

"I note here the interesting possibility that the ^{3}He produced in the pd fusion reaction may undergo a secondary reaction with another deuteron of the lattice, yielding ^{5}Li (an excited state of ^{5}Li lies close by). The latter is unstable against disintegration into a proton and ^{4}He. Thus, protons are not consumed in the overall reaction, which generates ^{4}He."

To this I add, as of some time in 1992, that observations of ^{4}He, with insufficient numbers to account for total heat generated, are consistent with the preceding suggestion. The initial pd reaction produces heat, but no ^{4}He. The secondary reaction generates heat and ^{4}He. There may be more total heat than can be accounted for by ^{4}He production. The smaller the ratio of secondary to primary rates, the more the ^{4}He production will be incapable of accounting for the heat generation.

The second part of "A Progress Report" is entitled: "Energy Transport in Sonoluminescence." What is that?

The text begins with:

"The suggestion that nuclear energy could be transferred to an atomic lattice is usually dismissed ... because of the great disparity between atomic and nuclear energy scales; of the order 10^7, say. It is, therefore, of great psychological importance that one can point to a phenomenon in which the transfer of energy between different scales involves (an) amplification of about eleven orders of magnitude."

"It all began with the sea trials, in 1894, of the destroyer HMS Daring. The onset, at high speeds, of severe propeller vibrations led to the suggestion that bubbles were forming and collapsing—the phenomenon of cavitation. Some 23 years later, during World War I, Lord Rayleigh, no less, was brought in to study the problem. He agreed that cavitation, with its accompanying production of pressure, turbulence, and heat, was the culprit. And, of course, he devised a theory of cavitation. But, there, he seems to have fallen into the same error as did Isaac Newton, who, in his theory of sound assumed isothermal conditions. As Laplace pointed out in 1816, under circumstances of rapid change, adiabatic conditions are more appropriate.

During World War I, the growing need to detect enemy submarines led to the development of what was then called (by the British, anyway) subaqueous sound-ranging. The consequent improvement in strong acoustic sources found no scientific applications until 1927. It was then discovered that, when a high intensity sound field produced cavitation in water, hydrogen peroxide was formed. Some five years later came a conjecture that, if cavitation could produce such large chemical energies, it might also generate visible light. This was confirmed in 1934, thereby initiating the subject of sonoluminescence, SL. I should, however, qualify the initial discovery as that of incoherent SL, for, as cavitation noise attests, bubbles are randomly and uncontrollably created and destroyed.

The first hint of coherent SL occurred in 1970 when SL was observed without accompanying cavitation noice. This indicates that circumstances exist in which bubbles are stable. But not until 1990 was it demonstrated that an SL stream of light could be produced by a single stable cavity.

Ordinarily, a cavity in liquid is unstable. But it can be stabilized by the alternating cycles of compression and expansion that an acoustic field produces, provided that sonic amplitudes and frequencies are properly chosen. The study of coherent SL, now under way at UCLA under the direction of Professor Seth Putterman, has yielded some remarkable results.

What, to the naked eye, appears as a steady, dim blue light, a photomultiplier reveals to be a clock-like sequence of pulses in step with the sonic period, which is of the order of 10^{-4} seconds. Each pulse contains about 10^5 photons, which are emitted in less than 50 picoseconds, that is, in about 10^{-11} seconds.

When I first heard about coherent SL (my term), some months ago (June 1991), my immediate reaction was: This is the dynamical Casimir effect. The static Casimir effect, as usually presented, is a short range, non-classical attractive force between parallel conducting plates situated in a vacuum. Related effects appear for other geometries, and for dielectric bodies instead of conductors.

A bubble in water is a hole in a dielectric medium. Under the influence of an oscillating acoustical field, the bubble expands and contracts, with an intrinsic time scale that may be considerably shorter than that of the acoustical field. The accelerated motions of the dielectrical material create a time-dependent dynamical electromagnetic field, which is a source of radiation. Owing to the large fractional change in bubble dimensions that may occur, the relation between field and source could be highly nonlinear, resulting in substantial frequency amplification.

The mechanisms that have been suggest-

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ed for cold fusion and sonoluminescence are quite different. (So I wrote in 1991.) But they both depend significantly on nonlinear effects. Put in that light, the failures of naive intuition are understandable. So ends my Progress Report."

In the more than two years that have elapsed since the birthday lectures, I have concentrated on the theory of coherent sonoluminescence. Why? Because, of the two physical processes that naive intuition rejects, it is coherent SL that exists beyond doubt. (No, Mr. Taubes, not even you could cry fraud. Too many people have seen the light.) With the advantage of reproducible data, under variable circumstances, constructing a convincing theory for coherent SL should be, by far, the simpler. That, in turn, should supply analogies for theory construction in a domain that is characterized experimentally by "irreproducibility and uncontrollable emission in bursts."

My gut feeling about the Casimir effect, in a dynamical role, first needed some brushing up in the static domain, which I had not thought about for 15 years. My progress in doing that, along with needed simplifications, is recorded in four notes, published in 1992. Two of them share the title "Casimir energy for dielectrics." Each note acknowledges the stimulation provided by the phenomenon of coherent SL. I give only this brief excerpt concerning the action quantity W_o:

"What the static and dynamic Casimir efects share is the reference to the quantum probability amplitude for the preservation of the photon vacuum state: (exponential of iW_o). That the vacuum persistence probability is less than one, in a dynamical situation where photons can be emitted, is expressed by a nonzero imaginary part of W_o: .... In a static situation where W_o is real, the shift in phase associated with a time lapse, ... identifies E, the energy of the system, ..."

In the latter part of 1992, and in 1993, five papers were submimtted under the cover title "Casimir light." The individual ones are called, successively: A glimpse; The source; Photon pairs; Pieces of the action; and, Field pressure. The first three notes adopted the over-simplification that the bubble collapse—the source of radiant energy—is instantaneous. "Pieces of the action" begins "to remove the more egregious aspects of that treatment." The Abstract reads:

A more realistic dynamics for the collapsing dielectric fluid are introduced in stages by adding contributions to the Lagrangian that forms the action. The elements are kinetic energy, Casimir potential energy, air pressure potential energy, and electromagnetic coupling to the moving dielectric. There are successful tests of partial collapse time and of minimum radius."

This paper ends with a veiled question:

"If, as it would seem, a mechanism exists that transfers kinetic energy of a macroscopic body into energy of microscopic entities, could there not be—in a different circumstances—a mechanism that transfers energy of microscopic entities into kinetic energy of a macroscopic body?"

What, in 1991, seemed to be only a pairing of two intuitively improbable phenomena ("The mechanisms that have been suggested for cold fusion and sonoluminescence are quite different."), now emerges as related ways of transferring energy between macroscopic and microscopic objects.

"Casimir light: Field pressure" begins with a question:

"How does a macroscopic, classical, hydromechanical system, driven by a macroscopic acoustical force, generate an astonishingly short time scale and an accompanying high electromagnetic frequency, one that is at the atomic level?"

In response, "I offer the hypothesis that light plays a fundamental role in the mechanism. Provocatively put:

The collapse of the cavity is slowed abruptly by the pressure of the light that is created by the abrupt slowing of the collapse."

The hypothesis becomes more quantitative with this supplement:"The conditions for light emission are at hand when the fluid kinetic energy becomes independent of t (time) for a short time interval, and that similar remarks apply immediately after the emission act. In effect, one is picking out the circumstances for spontaneous radiation, from a coherent state of definite energy, to another such state of definite, lower energy."

The equation of motion—along with the conservation laws–that is supplied by the action principle, leads to a picture of what happens during abrupt slowing." Just before that begins, there is no significant field .... Then the field strength rises rapidly in the vacuum region, giving a positive value to the (outward pressure).... the slowing has begun. That process will cease when the field, flowing at the speed of light toward the outer dielectric region, has produced the countering pressure."

The somewhat mysterious initial hypothesis has emerged clarified, as an unusual example of a familiar fact—spontaneous emission of radiation by an electric system is a single indivisible act that obeys the laws of energy and momentum conservation."

Now, finally, returning to the 1991 "Causal order" note, for the example of the reaction

$$p + d \rightarrow {}^3He + \text{lattice energy},$$

one also recognizes this as a single, indivisible act.

So ends this Progress Report.

Julian Schwinger

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Very Hot Cold Fusion:

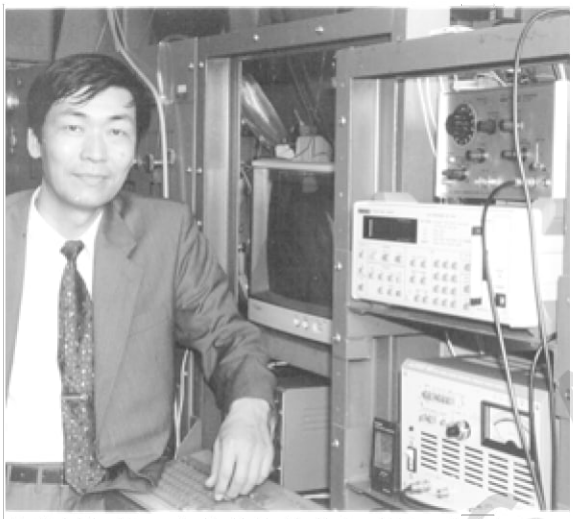
Dr. Mizuno's Ceramic Proton Conductors

by Jed Rothwell

Tadahiko Mizuno has been on the forefront of cold fusion from the very beginning. In the spring of 1989, he was one of the few scientists qualified to do this type of electrochemistry and also interested enough to try the experiment. He was ideally positioned to try a replication. He is an assistant professor at Hokkaido National University in the nuclear engineering department, where they have the kinds of detectors and instruments needed for this type of work. He is an electrochemist, who studied under John O'M. Bockris at Texas A&M University. He had electrochemical and calorimetric equipment set up and access to a specially shielded underground laboratory, with a three-meter thick lead door. This room has low natural background radiation, making it ideal for searching for neutrons. Years earlier, by coincidence he had done similar work with palladium and heavy water. He ruefully recalls that he once observed electronic noise that he now realizes might have been caused by nuclear events. At the time, he and his coworker looked for the source of the noise, thinking it was a bad solder connection or some other ordinary problem. They found nothing, and after a while they gave up the search.

Soon after the famous 1989 announcement, Mizuno set to work. A few months late, at a staff meeting, the head of the department suggested that "someone here should try this new Pons and Fleischmann experiment." Mizuno reported that the effort was already underway. A few months later he confirmed very weak levels of 2.45 MeV neutrons. This was one of the first verifications of the Pons-Fleischmann effect in Japan.

In May 1990 Mizuno began using closed cells in order to drive the D/Pd ratio higher. He used stainless steel cylinder 8 cm diameter, 20 cm long, 1000 cc capacity. In the upper portion of the cell he installed Pt mesh, which served as recombiner. This closed call allowed him to test high pressures and temperatures. On Feb. 22, 1991 he confirmed tritium production. He also confirmed that the number of neutrons produced by the cell was much smaller than the number the number of tritium atoms. During 60 days of electrolysis, he detected 3×10^{11} atoms of tritium, which was roughly 10^4 more than the number of neutrons observed during this same time period. With hot fusion, the numbers would be equal.



Dr. Tadahiko Mizuno in his Hokkaido National University Laboratory

In April 1991 he observed excess heat for the first time, in a gigantic heat event after turning off electrolysis. This phenomenon has since been reported by Pons and Fleischmann, McKubre, and others and it has become known as "heat after death." It was described in a dramatic narrative by Fujio Nakano in the *Bungei Shunju* magazine:

> "Mizuno set up an experiment on March 20th. He ran electricity through the platinum to the palladium electrode for six hundred fifty hours, up to April 22. Then he turned off the electricity and terminated the experiment. It was the same routine he had always followed. Then, three days later, on the morning of the 25th, he went to the basement laboratory. He reached out to touch the cell. He was astonished to find that it was extraordinarily hot. Far too hot to handle.
>
> Usually, the experiment ends when the electricity is cut. The heater that is wrapped around the cell is left on for a while, while measuring the background neutron radiation. The electricity going into the heater is controlled by a thermostat that keeps the cell temperature at 90°C at all times. However, just by holding his hand near the cell, Mizuno could easily tell that the temperature was way over 90°C.
>
> He could not decide what to do. It was clear that the heat was originating from inside the cell, but there was no electricity flowing through the electrodes, so he did not know what could cause the heat, nor could he control the heat. If he left it as it was, there was nothing to stop it from becoming dangerously hot. He turned off the heat wrapping.
>
> The temperature in the basement laboratory is maintained at 20 degrees C year round. Until now, whenever he turned off the heater, the cell would slowly cool down, until in about twenty minutes it would cool down enough to be handled with bare hands. But not this time. He waited an hour, then two hours, but it stayed far too hot to touch. Mizuno ran around the engineering labs, gathering towels. He wrapped the cell in a bundle of towels and carried it up to the third floor lab. He wrote a warning "HIGH TEMPERATURE DO NOT TOUCH," put the cell in a corner of the lab, and waited for it to cool."

Mizuno determined that over a 55-hour period from the 22nd to the 25th, the cell produced 792,000 joules of "heat after death" excess energy.

By this time, Mizuno was convinced that the cold fusion effect exists. He reported his results regularly at the Electrochemical Society meetings, where he was met with a barrage of harsh criticism and disbelief. He persevered in spite of the difficulties, the attacks, and lack of support. The university gave him the usual level of discretionary funds for the experiments, but it was never enough. Over the years, he spent quite a bit of his own money for laboratory equipment. Some people within the university still oppose the work.

After years of experimenting with it, Mizuno decided that the palladium in heavy water approach was unpromising. Results were sporadic at best and extremely unpredictable. It was difficult to load the metal to high levels and to maintain the high loading. In Maui, McKubre described just how unpredictable palladium is and how little we know about it: two adjacent sections of the same palladium

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wire will sometimes have completely different loading characteristics. Mizuno felt that there had to be a better way to achieve the CF effect. There must be some method of circumventing these problems. He doubted that high loading was as critical as others believed. He felt that a combination of moderately high loading plus vigorous movement, heat, and other disequilibrium conditions within the lattice could be just as effective as ultra-high loading. Furthermore, an ultra-high loaded cell kept in quiet, low temperature equilibrium may not show signs of a CF effect. Cold fusion seems to require both high loading and some kind of triggering effect. Perhaps when deuterons oscillate and move rapidly within a moderately loaded lattice they come together periodically to form areas of ultra-high loading. Whatever the reason, many people have noted that high heat or rapid changes in voltage will help trigger CF heat.

Around May of 1991, Mizuno first heard about the work of Eichi Yamaguchi at NTT. Yamaguchi reported that gas-loaded, thin film devices produce bursts of heat and helium-4. This radical new departure inspired Mizuno to try gas phase proton conductors. Gas loading is better than liquid electrochemistry in many ways: it is less complicated, it loads faster and more uniformly, it is less likely to introduce contamination. Electrolysis produces a steady stream of dissociated hydrogen and oxygen, which must be recombined in a closed cell, or vented with an open cell. This produces unexpected pressure changes, oxygen contamination, problems with recombiners, and a host of other aggravations. A gas-loaded cell is easier to use; less finicky, although gas cells do have their own special problems, like leaks which prevent high vacuums. Gas cells seem inherently more likely to become a practical source of thermal energy. It would be difficult run a liquid electrochemical cell at high temperatures, because the electrolyte would have to be pressurized, whereas a gas cell can easily be run at high temperatures and low pressure. In fact, the proton conductors must be run at high temperatures. Mizuno's devices barely conduct electricity below 300°C.

Mizuno began experimenting with proton conductors in the summer of 1991. He tried a series of strontium-barium devices, and then another series based on lanthanum and zirconium. He tried a variety of experimental protocols, but he got no trace of excess heat. He finally settled on the strontium-cerium material he uses today. It is worth noting, however, that the first 20 samples of strontium-based conductors did not produce any excess, and many of those he makes today still fail. This shows the importance of Edisonian science: you have to try and try again. Until a theory can be worked out to adequately explain the CF reaction, it seems likely that we will have to rely in many cases on hit-or-miss testing, in which only a fraction of the devices produce an effect, and the level of the effect varies widely from one device to another. This was the best that could be done in silicon chip production for many years.

Mizuno's choice of materials and formula, $SrCeO_3$, is related to that used with many high temperature superconducting devices. Intuitively, this points to some kind of link between superconductivity, magnetism, and CF. There are other hints of this link: at ICCF4 Cravens, Bockris, and others noted that sometimes a magnetic field appears to trigger a heat burst in a palladium cell.

Component Configuration

Mizuno's proton conductors are round, pink ceramic devices, about the size of a U.S. quarter. In this experiment, a conductor is placed in a vacuum chamber. It is placed in sample holder assembly. Figure 1 shows the configuration of that assembly from the 1993 experiments. The biggest item is a large copper block which acts as a sample holder and a heat sink. Four stainless steel bolts screw into the copper block. The bolts hold the conductor in a kind of sandwich: a nonconductive plate on the outside; a platinum electrode, the conductor, another electrode, a plate of mica insulation, a 1 mm thick nickel plate, and finally the copper block. The bolts hold these parts together, keeping the conductor snug against the nickel (although electrically insulated from it), and keeping the nickel snug against the copper. A thermocouple is attached to the nickel. The copper block has a series of holes drilled through it. An insulated nichrome heater wire is passed through the holes, back and forth, like a shoelace. The nichrome wire acts as a joule heater, and the heat from it is spread evenly through the copper, nickel, and across the insulator to the proton conductor. The heater serves two purposes: it acts as a calibration reference, and it heats up the proton conductor to a high enough temperature that it will conduct. The assembly is attached to rod which bolts it to the reactor vessel lid.

Most cold fusion experiments use this technique: assembled parts, thermocouples and other items are attached to the lid of the container, and then the whole rig is lowered into the reactor vessel like a ship in a bottle. This experiment requires a vacuum, so the top must fit precisely, with an o-ring seal between the lid and the vessel. Keeping this sufficiently airtight for a high vacuum is difficult. Mizuno has experienced many problems with minute, slow leaks.

The earlier experiments employed only one thermocouple in the cell. The cell used in the 1994-1995 work has three thermocouples on the sample and an additional thermocouple in the cylinder walls. Thermocouple readings, pressure, and other parameters were recorded with pen recorders in the older experiments, and on a computer in the more recent work. Temperature is recorded with 0.1°C accuracy. Gas pressure was measured with a capacitance manometer with 0.1 Torr accuracy.

Experimental Procedure

The sample holder is attached to the lid, the lid firmly closed, and the vessel is sealed and evacuated. The nichrome joule heater is turned on and left on for the entire experiment at some fixed value, usually 100 watts. The copper block, nickel plate, and proton conductor are all heated uniformly, because copper conducts heat so well. After 30 minutes the temperature of the copper block and conductor are stable, usually at about 380°C. One-tenth atmosphere of deuterium gas is introduced into the chamber, and a very low level of electric power is introduced into the conductor electrodes. Voltage ranges from 5 to 45 V, but amperage is low, typically less than 1.0 mA. It is a unipolar alternating current which is set at slow speed: between 1 Hz and 0.005 Hz (much slower than standard 60 HZ electric power). Total power into the conductor ranges from a fraction of a milliwatt up to around 20 milliwatts.

The introduction of the gas lowers the temperature of the sample, because the gas conducts heat to the vessel walls. With a vacuum, the copper block radiates heat slowly to the surroundings. When gas is introduced, convection and conduction increase the heat loss, and the temperature drops. Blank control runs show that the temperature in the cell will drop from 380° down to 270°C with 0.1 atmosphere of deuterium gas. (The nichrome heater keeps it from dropping to room temperature.) With hydrogen gas, it drops a little farther, down to about 250°C, because hydrogen conducts heat better then deuterium.

With a control run, after the temperature drops it remains rock steady. With a dead, non-functional proton conductor, it drops to the same temperature it does with a control run, and remains rock steady. The

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proton conductor has a few milliwatts of alternating current power going through it, but the instruments were not sensitive enough to detect such a low power level. A dead conductor settles at virtually the same temperature as a blank run with no conductor in the holder at all, because the milliwatt levels of power driving the conductor produce less than 0.1°C temperature rise, which is the smallest measurable temperature difference these instruments can detect.

A dead conductor settles at the calibration temperature. A particularly good live conductor begins to drop, but it then dramatically turns around and rises back up to the 390°C level, and sometimes beyond that, which demonstrates that there is dramatic excess heat from the conductor. The joule heater power level is unchanged; the cell is now filled with gas instead of a vacuum, and gas always dramatically lowers the temperature, yet the temperature climbs back up higher than it was before! The heat is not steady as it is during a joule heater calibration run, or with a dead conductor. Sometimes it is extremely erratic. Figure 2 shows one of the most dramatic examples of excess heat, in which the temperature actually exceeded the vacuum setting. The top curve shows the temperature with a strontium conductor ($Sr(CeYNb)O_3$) in deuterium (D_2) gas. The temperature initially drops at hour 1, but it recovers by hour 3, and the temperature then rises to roughly 400°C. The other two curves on this graph are for a dummy aluminum conductor in deuterium gas, which drops to 270°C; and a strontium conductor in hydrogen gas, which drops to 250°C. The difference in sustained temperature between the strontium conductor and the aluminum conductor is ~130°C, which is all caused by the excess heat from a reaction that occurs in the conductor. This represents about 50 watts of excess power, conservatively. This gigantic temperature continued for more than 20 hours, before finally petering out. During that period, the excess was 20 MJ (million joules). To put this in perspective, this coin-sized ceramic generated as much energy as you get from burning 500 ml of gasoline. A mere milliwatt of input power produced at least 50 watts of output, or 50,000 times input.

Most of the other results have not been this dramatic, although one run was altogether too good. As shown in Figure 3, the temperature soared to over 1600°C during the first hour. That melted solder connections within the cell, which cut off further temperature readings. The dotted line shows estimated temperatures after that. This heat burst peaked at greater than 150 watts.

Extensive Calibration

Mizuno performed extensive calibrations during these experiments. The full set of calibration runs lasted about a month. Each calibration point represents 5 to 6 hours of data at one power setting. Calibration was performed as follows:

Gas pressure set at either a vacuum, 0.1 atmospheres, or 0.2 atmospheres in some tests.

The nichrome joule heater is set at a given power level, say, 40 watts. After 30 minutes the heat-sink in the cell reaches a steady state temperature. The power level is held for 5 hours and the average temperature after the first hour to the end is computed.

Different power levels are selected: 50, 70, 110. Up to 13 data points for each pressure setting are taken.

Tests for both deuterium and hydrogen were performed. Control runs with a dummy aluminum conductor were performed. As noted above, these showed power input to the conductor was so small (being less than a milliwatt in most cases) that it could not even be detected with these instruments. The aluminum conductors were tested in both deuterium and hydrogen. Strontium conductors were also tested in ordinary hydrogen. They usually showed no sign of excess but occasionally a slight excess was seen with the 1994 instruments. This may be caused by the fraction of deuterium present in ordinary hydrogen samples (samples taken from ordinary water, not purified light water).

Doubling the gas pressure to 0.2 atmospheres did not change the heat transfer coefficient much. This means that minor fluctuations in pressure and inaccuracies in pressure reading have little effect on heat transfer coefficients computed during calibration.

Difficulties

Mizuno faces many continuing difficulties. The conductors are hard to make, requiring a painstaking process described in his ICCF4 paper:

> "Samples were made from a mixture of $SrCO_3$, CeO_2, $Y2O_3$ and Nb_2O_5 powders. Mixed oxide powder was once sintered in a furnace at 1440°C in air for 24 hours, and was pulverized, again mixed, and filtered to 400 mesh, The powder was pressed to form a plate 0.8 cm in diameter and 0.1 cm thick in a pressing machine and was once again sintered in a furnace at 1560°C for 24 hours. Both sides of the sample plate were then coated with porous Pt films, 0.05 ~ 0.15 m, by applying Pt paste and calcination at 700°C for 24 hours."

It takes about five working days to produce a batch of conductors. Mizuno talks about problems with furnace temperatures being a little higher than expected, problems with impurities introduced by mixing, and other difficulties, which give me the impression that producing a perfect batch of proton conductors is a great deal more difficult than the recipe for a 13-layer Hungarian dubish torte (which takes all day to make), or the most demanding French gourmet cooking. It is clear why he worked for two years without success. The work is probably comparable to making a set of silicon transistors with state of the art knowledge circa 1952. At that time, four years after the AT&T's Bell Labs had produced the first practical transistor, their manufacturing arm, Western Electric, was still having great difficulty manufacturing transistors. Only 5% of the transistors they manufactured worked, the rest had to be scrapped.

Mizuno describes many other difficulties like fighting leaks, and a never-ending battle with contamination from sources like impure deuterium gas. He is experimenting with new ways to manufacture and prepare the conductors and with new equipment configurations. For example, he wants to use infrared heaters instead of joule heaters, and a turbo-molecular pump. He thinks that his biggest problem is probably carbon oxide and carbon hydride contamination on the surface of the conductor.

When you see of how demanding this work is, and how much care it takes, it becomes obvious why nobody could replicate this work without a thorough study of Mizuno's technique, including many visits to his site. He has been working in close cooperation with other groups in Japan who have successfully replicated his work, but not without a struggle. People who oppose cold fusion research often claim that any scientific formula or device should be easily replicated by any scientist without special training. This is like saying that in 1948 any scientist should have been able to replicate AT&T's transistor results with a 100% success rate.

Mizuno Comments about the Implications of His Results

Mizuno estimates the deuterium flow rate through the conductor at 2.5×10^{14} atoms per 0.1 seconds. If every one of

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these atoms were to fuse in a conventional hot fusion D+D fusion reaction, they would produce roughly 500 watts, so if we assume that the reaction is fusion, which generates as energy by some mechanism similar to hot fusion, then roughly 10% of the deuterium fuel in the conductor is being fused and converted to helium. But the true nature of the heat-producing reaction remains unknown until the nuclear ash (if any) can be determined.

In his 1994 and 1995 work with more sensitive detectors, Mizuno has occasionally seen puzzling noise on the thermocouple closest to the conductor, but no noise on the other thermocouples attached to the other side of the heat sink. He has taken many steps to eliminate all sources of noise, but the bursts still appear from time to time. He now speculates that it is cause by charged particles originating in the conductor. Clearly, this very hot cold fusion work has much to offer the experimentalist, and it may provide a path to practical solid-state heating devices with vanishingly low input electrical power—power that could be generated by the self-sustaining (self-heating) reaction itself.

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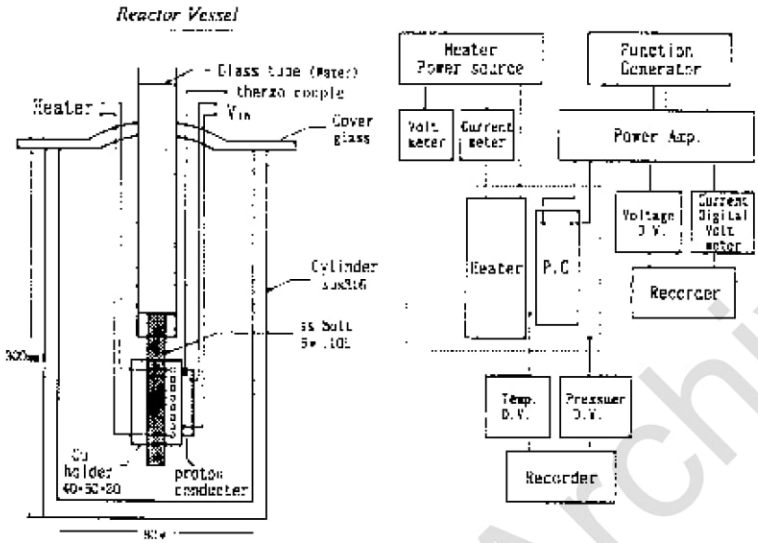

Figure 1. Cross-section of Dr. Mizuno's ceramic proton conductor test apparatus, 30 cm tall, at Hokkaido University. (Right) Schematic of instrurmentation. From ICCF4 Proceedings (Courtesy, EPRI).

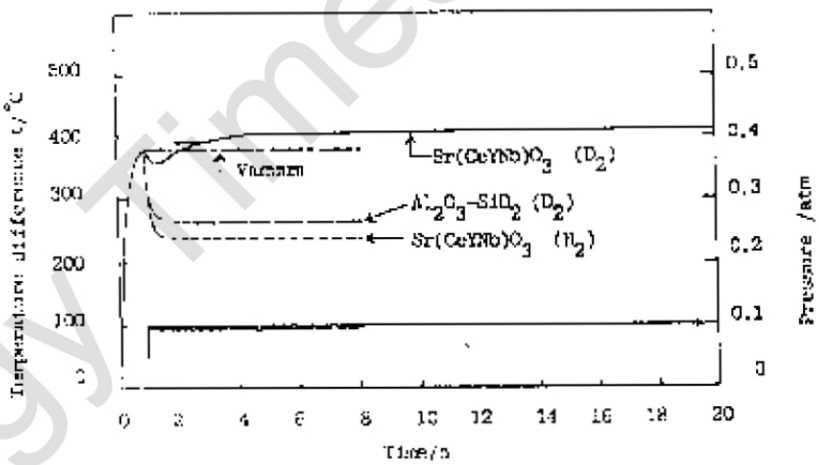

Figure 2. Temperature changes of proton conductor and dummy sample in various atmospheres under constant heater power input. The proton conductor in the deuterium gas atmosphere exceeds even the vacuum calibration line.

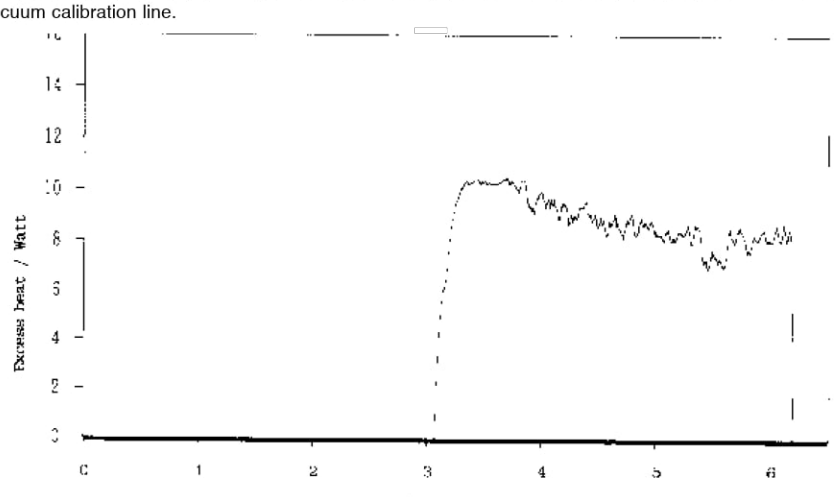

Figure 3. Excess heat changes in an experiment. The average excess heat evolution was 9 watts and lasted until the experiment ceased. Total excess energy estimated to be 10 kilo-joules. Courtesy, Dr. Mizuno.

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Cold Fusion Testing , 2/25-26/95, at Clean Energy Technologies, Inc.

MEETING REPORT

by Bruce Klein

LOCATION/PURPOSE

This meeting was held at Clean Energy Technologies' laboratory in Sarasota, Florida. The purpose was to witness the Patterson cold fusion cell in operation.

PARTICIPANTS

Clean Energy Technologies: Dr. James A. Patterson , Mr. James W. Reding

Vernon Jr. College, Texas: Dr. Dennis Cravens

Bechtel: Mr. Bruce Klein

BACKGROUND

Dr. Patterson holds four patents which relate to a cold fusion electrolytic power cell of his design. Two of the patents claim excess heat generation by the power cell. Clean Energy Technologies (CET) had scheduled testing for this weekend to independently verify the patent claims. The verification testing was done by Dr. Dennis Cravens, who is also a cold fusion researcher and who has submitted patent application(s) for devices of his design.

In addition to the patent verification testing, another test was run using a new variation on the design which was described in the patent. The variation has been found by CET to produce greater amounts of excess heat.

I had met Mr. Reding at the MIT cold fusion conference in January, 1995. When I expressed an interest in seeing CET's laboratory, and a cold fusion cell in operation, he invited me to witness and participate in the verification testing.

SUMMARY

During the two days of testing at CET, the following was accomplished:

- A cell using Ni/Pd beads was tested with heavy water electrolyte, and found to produce excess heat at a rate of approximately 130% (i.e. the cell was generating 30% more power than it was consuming). This confirmed the heavy water claims of the patents.

- A cell using Ni/Pd beads was tested with light water electrolyte, and found to produce excess heat at a rate of approximately 120% (20% more power produced than consumed). This confirmed the light water claims of the patents.

- A cell using Ni/Pd/Ni beads was tested with light water electrolyte, and found to produce excess heat at a rate of up to 250% (150% more power produced than consumed).

DISCUSSION

1. Description of the Patterson Power Cell

Overall Cell Design

The Patterson power cell is shown schematically in figures 1 and 2. It consists of a small glass electrolytic cell encased in a heat insulating cylinder. The cathode consists of many small spherical conductive beads sandwiched between a nonconductive and a conductive screen. The conductive screen is connected to the external cathode wire (negative electrode).

The anode consists of another conductive screen, which is connected to the external anode wire (positive connection). A layer of small, non-conductive spherical beads separate the anode and the cathode.

End plates connected by support rods hold the assembly together. Electrolyte flows in the bottom of the cell, past the cathode and anode, and out the top of the cell. Heat which is generated within the cell (by the energy of the impressed electrical cell current, and by the cold fusion effect) is transferred to the electrolyte and carried from the cell.

Not all of the heat which is generated by the cell is transferred to the electrolyte. Some of it leaks through the insulating cylinder, through the end plates, etc. To calibrate the cell to determine how much heat leaks from it, a calibration heater is embedded in the insulating block next to the cell.

The knurled nuts on the support rods permit the end plates to be compressed against the cell. This in turn compresses the cathode beads.

Operation of the Cell

Cold fusion electrolytic cells, including the Patterson power cell, operate in essentially the following way:

- An electrolyte is produced using either light water (H_2O) or heavy water (D_2O) and a salt. For the testing reported in this document, the salt used was Li_2SO_4 at a 1M concentration.

- A low current is applied to the cell. This causes hydrogen (H) or deuterium (D) from the electrolyte to dissociate and migrate into the cathode material. Only certain materials will admit hydrogen or deuterium, and the most commonly used for cathodes are palladium (Pd)and nickel (Ni).

- The low power is applied until the loading ratio in the cathode material is in the range of 0.7 to 1.0. This means that for every atom of cathode metal, between 0.7 and 1.0 atom of H or D has diffused into it.

- After a high loading ratio is obtained, the current to the cell is raised. This is generally sufficient to trigger the cold fusion effect, in which heat is generated within the cathode by an apparent nuclear reaction and released to the electrolyte.

The cold fusion effect is said to generate excess heat, because the heat transferred to the electrolyte is greater than that supplied to the cell by the external electrical power source. Excess heat of 130% indicates that 30% more heat energy flowed out of the cell than was supplied by the electrical power supply.

Cathode Beads

The feature of the Patterson power cell which distinguishes it from other electrolytic cold fusion cells is the use of small, electrically conductive beads as the cathode material.

The bead construction is shown in Figure 3. The Ni/Pd beads are the ones described in the patents. They consist of a plastic sphere, covered by a flash coat of copper (Cu). The copper is essentially a primer which permits a layer of Ni to adhere to the plastic. Finally, a layer of Pd is deposited over the nickel.

The Ni/Pd/Ni beads represent a refinement of the Ni/Pd beads. They consist of the same plastic, Cu, Ni and Pd layers, but they also have an outer layer of nickel.

CET feels that using beads as the cathode material provides the following beneficial properties:

- The layer of Pd is very thin. This permits the H or D to diffuse into it quickly, so the time required for loading is small.

- The beads provide a large surface area for a small volume. Since the operation of the cell and the cold fusion effect appear to be related to surface area, this is a beneficial property.

- The process used for depositing Pd on the beads results in a high purity layer of material. The cold fusion effect has been found to be very sensitive to material characteristics and imperfections, and achieving good material properties consistently is important to its success.

- Hundreds of beads are contained in the cathode. Even if a large number of these

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beads do not participate in the cold fusion reaction (due to imperfections or some other reason), the operation of the cell itself can still be a success.

- The compression on the beads by the end plates of the cell can be varied. This affects the electrical resistance of the cathode, and affects the operation of the cell. Typically, the knurled knobs are fingertight.

The new Ni/Pd/Ni beads provide the following additional benefits:

- The Ni layer helps to keep the Pd from spalling off the beads. When H or D diffuses into Pd, the Pd expands. This can cause the Pd to break off the beads. The nickel helps prevent this from happening.

- The Ni layer keeps the Pd in compression, which CET feels provides benefit for the cold fusion reaction.

The detriment of the outer Ni layer is that it takes longer to load the Pd (i.e. to diffuse H or D into it) because the diffusion rate through Ni is smaller than through Pd. Thus, for example, loading times of approximately 2 hours were required for the Ni/Pd beads, whereas loading over night was required for the Ni/Pd/Ni beads.

The cavity which contains the beads in the cells which were tested is 1.9 cm in diameter and 0.5 cm long. The total surface area of the beads in this volume is estimated by CET to be 32.5 cm2.

External Fluid Circuit
Figure 4. shows the external fluid circuit which was connected to the cells during testing. The actual test apparatus is shown in the photographs in figures 8 though 14 [*Not included in this article.—Ed.*]. The fluid system consisted of the following:

- A reservoir containing the electrolyte. If additional electrolyte is required during a test, it is added at this location.

- An adjustable metering pump to force electrolyte through the system at a known flow rate.

- A 47mm diameter, 8-micron cellulose acetate filter. This filter is intended primarily to collect Pd particles which spall off the beads.

- The cell itself. Flow enters the bottom of the cell and leaves from the top, so it is always filled with electrolyte.

- A graduated cylinder. By closing the outlet from the cylinder and timing the rate at which it fills, the electrolyte flow rate is determined.

- A vent on top of the graduated cylinder. Electrolysis of the water in the cell produces hydrogen, deuterium, and oxygen gas. This is vented from the system.

- A pressure gauge at the pump outlet, to indicate if clogging is occurring in either the filter or the cell.

- Thermocouples for measuring the temperature in the inlet and outlet of the cell. Thermocouples are monitored on a strip chart recorder.

External Electrical Circuit
Figure 5. shows the external electrical circuit which was connected to the cells during testing. The actual test apparatus is shown in the photographs in figures 8 though 14. The electrical system consisted of the following:

- Constant-current power supply. Integral with the supply are an ammeter and voltmeter.

- The cell itself. Voltage across the cell is monitored by a portable voltmeter, and is also displayed by the strip chart recorder.

- A calibrated resistance block which permits current to be displayed both on a portable ammeter and on the strip chart recorder.

- The electrical system associated with the metering pump is not shown, and consists only of a manual speed adjustment knob.

2. Method of Calculating Excess Heat
Refer to Figure 6. when reading this discussion.

Determining Cell Thermal Efficiency
The first step in each test is to determine the thermal efficiency of the cell being tested. This is defined as:

ET = Thermal Efficiency =

Power Carried Away by Electrolyte/
Power Generated in the Cell

This is done by applying a known voltage and current to the calibration heater, without any other power being supplied to the cell. This is then the power generated in the cell:

Power generated in the cell = (V) x (I)

The power carried away by the electrolyte is determined from the flow rate through the cell and the temperature difference across the cell:

Power carried away by electrolyte = (flow) x (temp. difference) x (0.070)

Note: Flow rate is expressed in ml/min.

Temperature difference is expressed in degrees C.

0.070 is a conversion factor to express power in watts.

As shown later in this document, the thermal efficiency for the cells tested ranged from 0.17 to 0.32.

Determine Output Power
After the cell has been loaded and is operating, the total power leaving it consists of:

Electrolyzed water gases + Power to heat electrolyte + Power leakage from cell

The volume of electrolyzed gas is not measured. Instead, the input power required to perform the electrolysis is subtracted from the total input power. (In other words, the power provided to the cell to perform the electrolysis is not counted in the calculation, nor is the power carried away by the electrolyzed gases. This is a conservative assumption because the gases also carry away heat and vapor, i.e. they carry away more power than required only to electrolyze them.)

So, power output from the cell is then:

Power Output = Power to heat electrolyte + Power leakage

Power leakage does not need to be measured, however, because it has been accounted for in the thermal efficiency of the cell, which was determined earlier. Therefore, the power output from the cell can be determined by:

Power Output =
Power of Heated Electrolyte/ ET

The problem with the Patterson cells tested this weekend is that the thermal efficiencies are quite low. Therefore small changes in the measured values have substantial impacts on the final results.

Determine Input Power
Total input power to the cell is merely (V) x (I). However, as described above, neither the input nor output electrolysis power is counted. 1.5 volts are required to electrolyze water, and the remainder of the voltage contributes to heating. Therefore the adjusted power input is:

Power Input = (V - 1.5) x (I)

Determine Total Cell Efficiency
The cell efficiency (i.e. a measure of the excess heat being generated by the cell) is then:

Cell Efficiency =
Power Output/Power Input

3. Test Methods
Attachment 1 consists of the test protocol specified by CET. This protocol falls within the parameters specified in the patents for Patterson power cell. The protocol consists of the following major steps:

A. Thermal Calibration
A thermal calibration of the cell is performed using the tungsten wire heater.

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The protocol calls for a calibration current power of 5 watts; calibrations were actually performed with 10 watts of input power.

B. Alpha-Phase Charging
A low-power charging period (0.02 amps for at least 1 hour) to load the palladium with H/D through the alpha phase of the palladium. Refer to Figure 7 for a phase diagram for palladium. At the end of the low-power charging run, the palladium is at approximately a loading ratio of 0.7.

C. Beta-Phase Charging
An increased-power charging period (0.1 amps for at least 1 hour) to drive the palladium into the beta phase.

D. Excess Power Production
A high-power run (10 watts, which required a current flow of approximately 1.5 amps) to initiate excess heat production.

The apparatus tested varied from that described in the patents. The major difference is that the test apparatus is larger: 1.9 cm cell inner diameter, vs. 0.9 mm described in the patent. CET's description of the differences are included in Attachment 2.

Obviously, before the test can be run, the beads must be loaded in the cell and the cell assembled for operation. CET's procedure for doing this is included as Attachment 3.

As described later in this report, three tests were performed. The first two were performed to verify operation of the cell as described in the patents, and the protocol above was adhered to. The third test was run to determine the performance of the new bead design, and operating parameters were varied to see cell response under different conditions.

4. Test Results

Ni/Pd Beads with Heavy Water
The first run was performed with Ni/Pd beads as described in the patents, and a heavy water (D_2O) electrolyte solution with 1M Li_2SO_4. The official records of the test are included in Attachments 4 and 5. The following conditions were imposed, and results observed:

A. Thermal Calibration
The thermal calibration data is shown in attachments 4 and 5 on the "Heater" line. A thermal efficiency of the cell (ET) of approximately 0.32 was measured.

B. Alpha-Phase Charging
Charging was performed at 0.02 amps. At the start of charging, the voltage induced across the cell was 2.71V, indicating a palladium resistance of 136 ohms.

After 45 minutes at 0.02 amps, the cell voltage had risen to 3.15V, indicating a cell resistance of 158 ohms.

After 88 minutes at 0.02 amps, the cell voltage had dropped slightly to 3.13V. The resultant drop in cell resistance to 157 ohms indicated the palladium had entered to beta phase, and beta-phase charging was initiated.

C. Beta-Phase Charging
Charging current was raised to 0.1A. A cell voltage of 3.78V was induced. After 75 minutes at 0.1A the cell voltage had stabilized at 3.87V. Excess power operation was initiated.

D. Excess-Heat Production
Current was raised to 1.5A. Excess power operation at two different points was documented, as shown in Attachments 4 and 5 on the "Heat Run" line. Cell efficiencies of 134% and 117%, respectively, were calculated.

Ni/Pd Beads with Light Water

The second run was performed with Ni/Pd beads as described in the patents, and a light water (H_2O) electrolyte solution with 1M Li_2SO_4. The same cell was used, although it was re-packed with new beads. The official records of the test are included in Attachment 6. The following conditions were imposed, and results observed:

A. Thermal Calibration
The thermal calibration data is shown in Attachment 6 [*Not included in this article.—Ed.*] on the "Heater" line. A thermal efficiency of the cell (ET) of approximately 0.30 was measured.

B. Alpha-Phase Charging
Charging was performed at 0.02 amps for one hour. At that time the cell voltage was 2.72V, for a cell resistance of 136 ohms.

C. Beta-Phase Charging

Charging current was raised to 0.1A. A cell voltage of 3.78V was induced. After 50 minutes at 0.1A the cell voltage had stabilized at 3.84V. Excess power operation was initiated.

D. Excess-Heat Production
Current was raised to 1.44A. Excess power operation was documented on the "Heat Run" line of Attachment 6. A cell efficiency of 120% was calculated.

Ni/Pd/Ni Beads with Light Water
The third run was performed with the new Ni/Pd/Ni beads and a light water (H_2O) electrolyte solution with 1M Li_2SO_4. A different cell was used. The official records of the test are included in Attachment 7, although many different conditions were examined. The following conditions were imposed, and results observed:

A. Thermal Calibration
The thermal calibration data is shown in attachment 7 on the "Heater" line. A thermal efficiency of the cell (ET) of approximately 0.17 was measured.

B. Alpha-Phase Charging
Charging was performed at 0.02 amps for 9 hours. At the start of charging, cell voltage was 2.77V for a cell resistance of 139 ohms. At that end of charging the cell voltage was 3.14V, for a cell resistance of 157 ohms.

C. Beta-Phase Charging
Beta-phase charging was done directly at a high current, approximately 2.2 A. This resulted in extremely unusual behavior of the cell for the next hour. Both voltage and cell temperature dropped during this period. Voltage dropped from 8V to 3.5V, and the temperature difference dropped to less than 1°C.

Calculations indicated the cell was endothermic rather than exothermic. CET had never seen this effect before, although Dr. Cravens stated that he had, and that in his experience this can be a precursor to particularly good performance of the cell during excess heat production.

The thermocouples were recalibrated at this time to correct for that possible source of error.

D. Excess-Heat Production
Various different conditions between 2.8 and 2.95 amps were imposed. Cell efficiencies between 107% and 258% were measured and calculated.

The cell was recharged at 0.02A for 1< hours at 0.02 amps, with thiurea added to the electrolyte. Dr. Cravens has observed that thiurea can accelerate the Pd loading process. At the end of charging, the cell voltage was oscillating between 0.5V and 1.0V.

In the process of measuring flow rate, the water flow was inadvertently interrupted to the cell. The power source reached its maximum conditions of 0.4A and 10V, and the temperature difference across the cell rose to 22°C.

Because of the flow interruption, temperatures inside the cell must have become very high, because when the cell was disassembled the nonconductive screen was found to have melted.

Several excess-power measurements and calculations were performed after this, with cell efficiencies ranging from 150% to 270%. The officially recorded efficiency in Attachment 7 was 174%.

5. Test Commentary
CET was surprised by the low-excess-

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power levels which were measured in the patent verification runs. Although they were sufficient to validate the patents, CET states they regularly get much higher yields. They concluded that the problem was due to the following:

- The cells had been loaded with too many nonconductive beads. This raised the cell resistances to higher levels than they normally experience.

- To keep the excess-power input level at 10 watts with this high-resistance bead layer, current was limited to only 1.5 amps.
- 1.5 amps did not provide sufficient current density for the palladium beads. The test protocol called for 2.6 amps, because their experience is that a higher current density is required for good excess heat production.

The calorimetry associated with the test setup at CET is fairly imprecise, and the heat losses from the system are large. This likely results in comparatively large errors associated with the measurements. Although excess heat production seemed to clearly be produced, the exact amount was not determined with great precision. Dr. Cravens is going to test a cell at his laboratory under more carefully controlled conditions.

If it is assumed that the size of the CET device is comparable with other cold fusion cells, then they share a common drawback: small size. Consequently the powers produced are small, and all calculations involve the differences between small numbers. To address this concern, CET is intending to scale their cell up to larger sizes. The device tested during this testing already represents their first scale-up of the cell.

FOLLOW-UP ACTION
As mentioned above, Dr. Cravens took a Patterson cell with him, to test in his own laboratory.

After he has completed this testing, he will assemble the cell and test components in a portable enclosure for demonstration at ICCF-5 in Monaco.

Prepared by Bruce Klein 3/4/95

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A Report on Testing the Patterson Power Cell technology

by Dr. Dennis Cravens
March 27, 1995

I was asked by Clean Energy Technologies, Inc. (CETI) to help verify and check the reproducibility of their Patterson Power Cell™ (PPC™ or Clean Energy Cell, CEC). I have had only a few months to run the experiments, prepare a talk, and assemble a demonstration unit for ICCF5 (the Fifth International Conference on Cold Fusion). Please excuse my brevity and style in this write-up. Expect more details and information at ICCF5 and after.

We decided to go through several steps in verification of the PPC. First, I traveled to Jim Patterson's lab in Florida and I watched him as he ran through the experiment. These were giving between 1:1 and 2:1 ratios for the total power out versus power in. Most of the experimental runs were done with 1.0 molar lithium sulfate solution of normal water. However, a few heavy water runs showed slightly larger excess ratios.

The second stage was when I traveled again to Patterson's lab and did the experiment with his equipment and guidance. I again got what appeared to be excess power ratios between 1:1 and 2:1. Bruce Klein was there for that experiment and he has a nice trip report on those experiments. The problem, at that time, was that the cell had a large heat loss—only 20 to 30% of the heat from the calibration resistor was detected in the heat gain of the fluid flow.

The third stage was for me to assemble and run the cell at my own lab in Texas. This was done in parallel fashion following the example from the experiment at Patterson's location. After getting nearly the same results, I have altered the system by placing the calibration resistor in the fluid flow and installing better insulation. This improved the heat retention of the cell from 20% to between 85 and 93% (depending on the sealing of the cell and packing). I then fully characterized my redesign cell by calibration over a wide-range of conditions (flow rates, current levels, inlet temperatures, etc.).

Initially, I had problems with unshielded thermocouples, but they were solved by stainless steel wells and common grounds. Their readings are now backed up with both thermistors and mercury thermometers. It now appears that the system is very robust and forgiving. It generally gives power ratios above 1:1 — even without correcting for heat loss or gas production. These uncorrected and high levels are very important. This means that many arguments against CF can be swept away, i.e. recombination, heat loss/gain, etc.

The next step will be to have the system analyzed for "nuclear ash." CETI hopes to sample for helium by the end of the year. CETI will make a wide range of information about the cell available at ICCF5 in hopes that we can teach others how to replicate the experiment. Equipment specifications, protocol, information on availability of materials, etc. will be made available at ICCF5. I hope to have (but *cannot promise*) a prototype at the conference—but that will depend on politics and logistics. CETI at this time is holding exact method of bead production private. Interested parties should contact CETI or ENECO about licenses. They will be seeking research and corporate cooperation to investigate the properties of their cells and beads.

The PPC™ is basically a cell using special beads as its cathode, a very small electrode separation and a flowing electrolyte. The beads I have been examining are sandwiches of Ni, Pd, Ni plated on styrene beads. The cell uses a 1.0 molar lithium sulfate electrolyte. A nylon screen separates the electrodes. The anode is made of a lithium salt saturated exchange resin material. The cell is first loaded with about 0.02 A of current for 4 to 12 hours. When the overall cell resistance increases (from 130 to 150 ohms, typically) loading is complete.

The input power is determined by the volts and amps supplied to the cell. The output is determined by the temperature change of the electrolyte flowing through the cell. Using just those two figures, heat produced is normally over 1:1 as calculated from "raw heat power out"/"raw electrical power in." I will be giving exact figures at the ICCF5 conference— we want it to be a pleasant surprise. It is important to notice that these are "raw power" numbers. This is the most conservative approach possible. Use of heat loss (via the cell's 85% efficiency) of the cell the power inherent in the gas production (1.5*I) serve only to increase those ratios.

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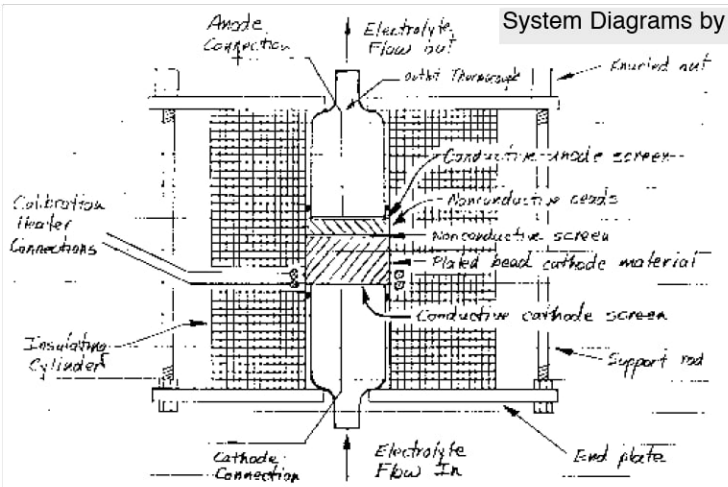
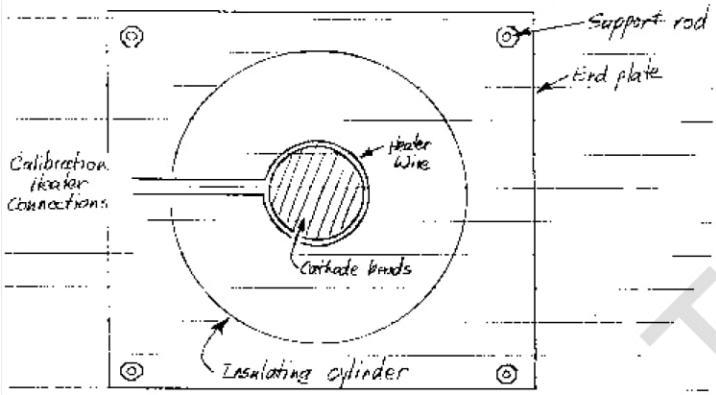
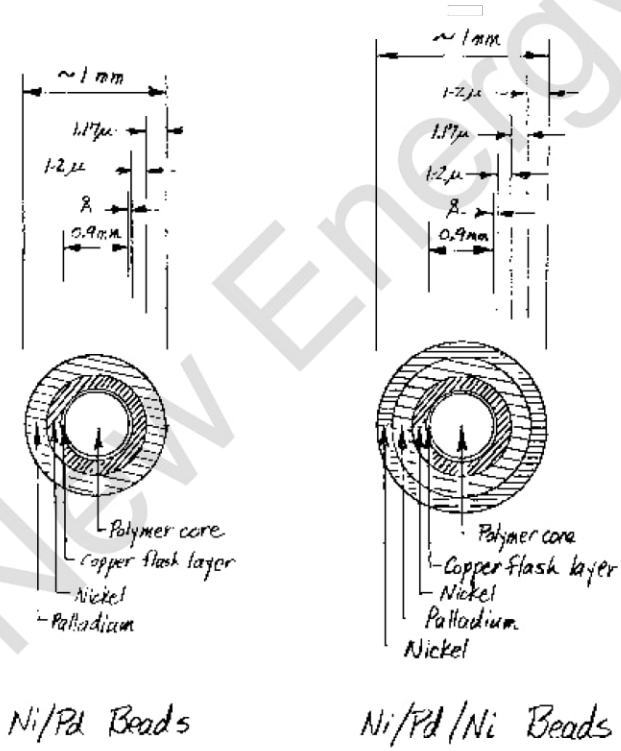
System Diagrams by Bruce Klein



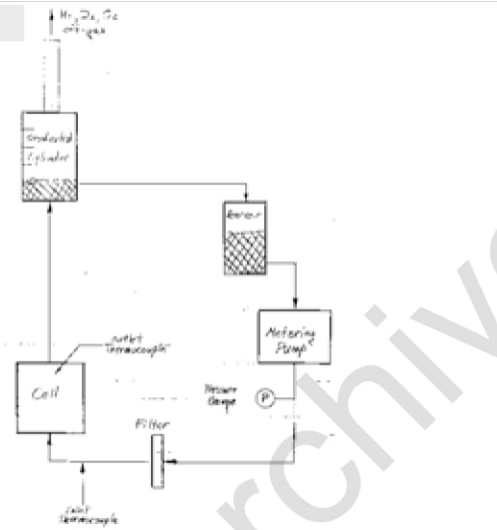
1. Patterson Power cell -- Schematic Cross Section



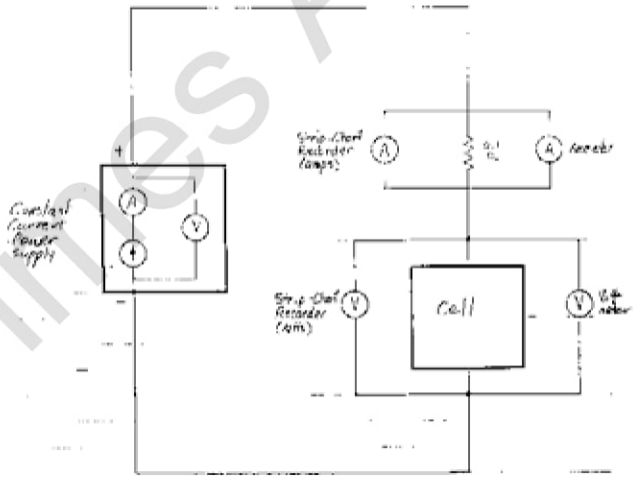
2. Patterson Power cell -- Cross Section Through Cathode Bead Area



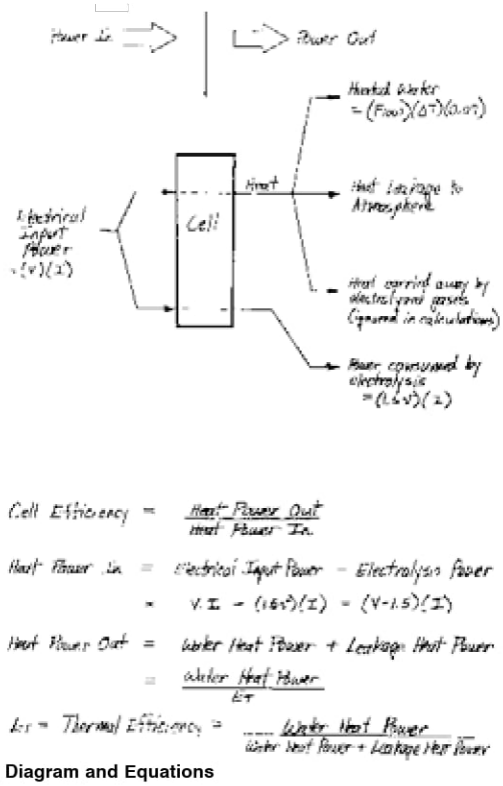
3. Cross Section of Cathode Beads



4. Test Apparatus Fluid Flow Diagram



5. Test Apparatus Electrical Schematic



6. Power Flow Diagram and Equations

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COMMENTARY

Cold Fusion: From Reasons to Doubt to Reasons to Believe

by Dr. Edmund Storms
Los Alamos National Laboratory
(Retired)

Six years have passed since the modern era of cold fusion was started by Professors Stanley Pons and Martin Fleischmann, both then at the University of Utah. During this time, criticisms made by skeptics have been taken seriously, errors have been reduced or eliminated, and a wide variety of studies have been done using very modern equipment in many countries. The early problem of reproducing the effect has been largely eliminated, the nuclear ash has been found, and theoretical explanations abound. The problem now is more psychological than scientific. In spite of all this new and improved information, general skepticism about the effect continues within the scientific community, and general rejection by the U.S. and many other governments remains unchanged.

I will not try to change this skepticism. To do so would take too much time and give many readers a bad case of boredom. Instead I will show those of you who have an active curiosity where the field now stands. If I succeed in arousing your interest, many sources of good information are now available to deepen your knowledge.

What is "cold fusion"?

The term now describes a general ability to produce a variety of nuclear reactions in addition to fusion. These reactions occur within solid matter near room temperature using a variety of methods . A better name might now be "Chemically Assisted Nuclear Reactions (CARN)." Use of super-high temperatures, powerful accelerators, or nuclear reactors are no longer the only methods needed to modify the nucleus or to extract energy. The original —now classic—electrolytic method used by Pons and Fleischmann is said to produce the "Pons-Fleischmann Effect." At least nine other methods have been found to be successful.

I should point out as an interesting aside, that part of this effect was claimed over 1700 years ago by what we call the alchemists. This group flourished in many countries for over 1300 years by impressing kings with their ability to create wealth, so we are told. The ideas proposed by this group were rejected when the modern scientific method was adopted and became successful. One wonders what else has been rejected from the past that should be reexamined?

Why should we care if this unexplainable and sometimes difficult to initiate phenomenon is real? We should care because this discovery has the potential to change the basic economic, social, and scientific fabric of our time. This wake-up claim is possible because energy can be created using an abundant natural resource without producing pollution, carbon dioxide, or radioactivity, and without the need for huge plants that require large capital investment. Granted, these claims seem too good to be true. Nevertheless, the potential importance should give you an incentive to hear the arguments.

Why is the effect impossible?

Nuclear interaction is restricted by the presence of a barrier created by the positive charge residing in the proton, a major constituent of all nuclei. This is the so-called Coulomb barrier. In order to move two nuclei close enough to interact, considerable energy must be invested. Many calculations using conventional theory have shown that, although a little fusion might take place under low-energy conditions, the amount required to produce the claimed heat or even the claimed tritium is impossible. Even if these calculations were wrong and the heat or tritium were caused by fusion, certain final products would be expected. These products include neutrons and tritium in large and equal amounts, the emission of gamma-rays from neutron interaction with the environment, and x-rays from excited electrons. All of these features are observed, but the quantities are too small and in the wrong proportion. In addition, tritium resulting from a fusion reaction normally has sufficient energy after formation to fuse with any deuteron it encounters. This reaction produces 14 MeV neutrons, yet no neutrons with this energy are observed. These arguments have been the basis for rejecting the claims and, on some occasions, they have blinded people to other possibilities.

What is the evidence for this unusual effect?

Tritium, neutrons, gamma rays, and excess energy have been seen under conditions that should not produce these reaction products.

Rejection of tritium as a nuclear product assumes that the palladium, cell material, or environment were contaminated before or during the experiment. Thus, the presence of tritium is acknowledged, but it is proposed to come into the cell by conventional means. This idea is no longer valid. Studies have now been done using sealed cells constructed from materials demonstrated to be tritium-free. A variety of methods have produced this isotope, and a variety of methods have been used to demonstrate that tritium is not initially present.

Neutron emission has been rejected because it is produced in bursts much like electrical noise or neutrons created by cosmic rays, and is close to background levels. New detectors and techniques have demonstrated repeatedly that neutron emission is real and, on several occasions, have measured an energy near 2.45 MeV . This is the energy expected from a fusion reaction. Additional neutrons at higher energy suggest an additional, complex reaction. Clearly, more than conventional fusion can occur. However, neu-

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trons are never emitted at rates necessary to originate from heat or tritium-producing reactions.

Gamma-ray detection has demonstrated the presence of radioactive byproducts resulting from several nuclear reactions other than fusion. These strange nuclear reactions have been seen occasionally after electrolysis as well as after using gas discharge techniques. It is very hard to explain away easily detected and short-lived radioactive decay in a metal that, previous to the treatment, was not radioactive.

Heat production implies a nuclear reaction at least 1,000 times faster than the best tritium production rate so far observed. Yet anomalous heat produces very little radiation and does not appear to require the production of tritium. Rejection of excess energy was, therefore, based on the absence of expected nuclear "ash" and radiation. Many early heat measurements were done using so-called open cells, i.e. ones that allowed the evolving D_2 and O_2 to leave the cell. Such cells can easily produce significant error. In addition, records were frequently not kept before excess energy production started. These deficiencies allowed skeptics to create possible explanations for the claimed excess involving unexpected recombination of the gases or unrecorded energy storage and release processes. Gradually heat measuring techniques were improved. Closed, sealed cells that allow nothing to enter or leave the cell are now studied. Some cells have produced a power equivalent greater than 3 kW /cm^3 of palladium and total energy greater than 200 MJ. This energy is greater than that released from a cell filled with exploding TNT. Of course this enormous energy and power were produced over several weeks of observation using small electrodes, thus avoiding the inevitable mess from an explosive release. Furthermore, no indication of unusual chemical reaction products has ever been found. In other words, the chemical "ash," required of a chemical explanation, is missing. If this energy is not produced by a nuclear reaction and is not generated by a chemical reaction, what is left? This alone should generate much interest. Recently, some very difficult studies have borne fruit and the nuclear ash has been identified

What is the nuclear ash?

The high temperature fusion reaction of deuterium nuclei is known to produce normal helium on rare occasions. When this reaction occurs in a high temperature gas (technically, a *plasma*), a gamma-ray must be emitted to carry away some momentum of the colliding deuterons as well as some released nuclear energy. The absence of such gamma-rays provided skeptics reason to reject this reaction. However, if we assume the entire crystal lattice can take away the energy, gamma emission should not be required. To test this idea, the presence of helium has been sought. Unfortunately, this measurement is very difficult because the amount of helium produced is similar to the amount found in air as an impurity. Skeptics can claim the observed helium resulted from air getting into the system. Over the years observations have been reported that shows the presence of helium only when heat was produced, and these observations have become increasingly accurate. Two reports stand outs, one by workers at the Naval Air Warfare Center, China Lake in California and the other at the University La Sapienza and the Laboratory of Physics in Rome. These studies have shown a close, although not perfect relationship between the amount of heat produced and the amount of helium detected. The latter work even shows all expected time delay between heat production and helium release. In addition, charged particle detectors located near palladium saturated with deuterium have, on occasion, detected the emission of high-energy alpha particles (4He). Various methods have shown these particles are not the result of impurities in the palladium and are only produced when deuterium is present. Thus, the original, high-energy reaction product has been seen as well as the accumulated helium after it has lost its initial energy.

Errors in both heat and helium measurements do not yet permit a conclusion that helium is the only source of heat. Nevertheless, helium is clearly present and a major source of anomalous energy.

What other strange behaviors have been seen?

The initial studies by Pons and Fleischmann revealed only the tip of an iceberg. At least seven methods and seven different chemical environments have been found to produce the effect when combined in various ways. Some combinations of environment and method produce nuclear reactions other than fusion. For example, when nickel is electrolyzed in a solution containing potassium carbonate (K_2CO_3) dissolved in normal water, excess energy is produced. This observation has been reproduced by at least nine different laboratories. Evidence from several of these laboratories shows that the potassium is being converted to calcium by taking a proton (hydrogen) into the nucleus. A similar reaction is found to convert rubidium to strontium. Potassium has a Coulomb barrier 19 times greater than that of deuterium and rubidium's is 37 times greater. This experience implies that the mechanism causing penetration of the Coulomb barrier can be very effective, indeed.

A gas discharge technique developed in Russia has given evidence that palladium will fission (will split into smaller nuclei) when bombarded with deuterons of the correct low energy. This reaction, combined with the now expected fusion reaction produces significant excess energy, gamma radiation emitted by several newly-formed radioactive isotopes, and changes in the isotopic ratio of newly produced nonradioactive elements. Efforts to replicate this work at several major laboratories are ongoing and have been partially successful.

Certain complex oxides ($SrCeO_3$ for example) loaded with deuterium become electrical conductors at high temperatures because the D^+ ions can move within the material. When a small current is applied, significant excess energy is produced.

A technique using high-intensity ultrasonic frequencies has been used by E-Quest Sciences in California to

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load deuterium into palladium from heavy water. Large levels of excess heat production are claimed and this heat is accompanied by easily detected helium production. This method is important and is being examined by several laboratories.

This is only a brief list of a few relatively well documented claims. The list of examples showing totally unexpected nuclear reactions and/or energy generation is growing. Even the imaginations of cold fusion supporters are being overwhelmed by some of the claims.

What about an explanation?

Two aspects must be explained to achieve a useful explanation. These aspects involve the special and rare conditions that must be achieved in order to initiate a nuclear reaction, and the nature of that nuclear reaction.

First of all, the necessary conditions are not easy to achieve and probably involve only small, isolated parts of the bulk material. In addition, the various nuclear reactions are initiated by different conditions within the same material and by different materials. Therefore, a universal characteristic is difficult to identify.

Once the special conditions are created, the Coulomb barrier might be reduced by various processes now being explored. Over 100 theories have addressed this problem with some success. Most have tried to extend conventional theory, while a few have chosen to explore new territory

Once a nuclear reaction occurs, the absence of significant gamma- and x-radiation suggests that the energy is coupled to the entire crystal lattice rather than to an individual reaction product. Although this energy coupling is outside of conventional explanations, over a dozen new theories have been proposed.

These new insights into nuclear and solid-state behavior, stimulated by the Pons-Fleischmann Effect, are expected to have a profound effect on our view of electron-nucleus interaction. Methods to form novel materials will be suggested regardless of how the models are applied to cold fusion.

What general interest has this evidence created?

Eight international cold fusion conferences have been held and several professional societies have included sessions about cold fusion at their meetings. The literature on the subject has grown to over 1,300 publications, some peer-reviewed. Clearly a lot more is known than many skeptics realize. A journal of the American Nuclear Society, *Fusion Technology*, has had a cold fusion section since September 1989, a newsletter called *Fusion Facts* has been published since 1989, a magazine called *"Cold Fusion"* was started in 1994 and then became a newsletter, a *Cold Fusion Times* newsletter has emerged, *21st Century Science and Technology* has long had regular articles on cold fusion, and a new magazine, *Infinite Energy —Cold Fusion and New Energy Technology* has just been founded. Occasionally, the general print and television media have acknowledged continuing interest in cold fusion, sometimes with objectivity and sometimes not.

A few governments and a growing number of companies are funding work in the field. Support in Japan is estimated to be at least $30 million per year and is known to involve universities, private, and government laboratories. A Japanese company is presently funding the work of Pons and Fleischmann near Nice, France. Acceptance extends to high government levels. Clearly, Japan is intent on understanding and eventually using this new phenomenon.

Support is growing in Italy, India, China, and, until recently, in Russia. Low level, isolated work is underway in several other countries, generally in industrial laboratories.

The U.S. government has shown little interest in supporting work even though several government laboratories have obtained evidence for the effect. The U.S. patent office will not issue patents that use the term "cold fusion" even though over 250 applications have been received. A few companies are supporting work, the most notable being the Electric Power Research Institute (EPRI) and more recently ENECO. Several companies have been formed to manufacture useful items for the field and to develop unique ideas or techniques. Small, isolated efforts are underway in a few industrial, government, and university laboratories, as well as in a few private homes. The total effort in the U. S. probably does not exceed $3M/year and may be dropping, though this is difficult to assess. Because theory is lacking, training of workers involved in research or commercialization has to be done using shared personal experience. This kind of knowledge is not easily available to a group or country that has not created a basis for obtaining such experience. Furthermore, no more than 100 scientists in the the U.S. have sufficient understanding of the field to make a meaningful contribution. Therefore, experience obtained in Japan and elsewhere will be slow to benefit development efforts in the U.S. or in other countries taking a similar approach.

What are the consequences?

The phenomena of cold fusion have been demonstrated over and over again. The remaining question is, can the effect be amplified to industrial levels? If such amplification is possible— no show-stoppers have yet been seen— this energy source will eventually replace most present energy sources, starting in Japan, followed by the Third World. Little imagination is needed to predict what this change would mean to countries that ignore this energy source. Is it wise to take such risk just because a few influential scientists have seen fit to reject a growing body of positive results? Would it not be better to risk a little money in case those skeptics are wrong?

Another implication involves the approach taken by some skeptics. While there have been good reasons to doubt the reality of some claims, the vicious and hostile attacks directed toward many in the field have no place in normal science. No useful contribution is made by deriding efforts to understand a phenomenon by calling it "pathological science" or the like, and characterizing the investigators as incompetent. Which viewpoint do you consider the worse sin: perhaps wasting some time and money understanding a phenomenon

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that later turns out to be trivial, or preventing the study of a phenomenon that later turns out to be important? A climate emphasizing caution and fear-of-failure does not bode well for our future efforts to discover and develop new gifts of nature.

Why Technology First

by Dr. Peter Glück

The Problem

"We are very good at making and talking about the bricks of the temple of science, but most of us are shy about the mortar or about the speculative blueprint of the whole design" — Gerald Holton

"The great menace to progress is not ignorance but the illusion of knowledge" —(Author unknown, quoted after Daniel Boorstin)

I will present here my vision about Cold Fusion, an offer for a strategy in this field, and a theory. These are as subjective as possible because I have learned well that an "objective analysis" is both a contradiction in terms and a symptom of fear of responsibility or of inadequate knowledge. I don't aim to *compete* with the many other theories which try to explain the puzzles of Cold Fusion; obviously there is a state of crisis which requires a radical change of rules in the contest, that is what Edward de Bono has called *surpetition*, creatively replacing the usual "competition" [1]. The trick is not how to defeat the "rival" theories but how to assimilate selectively their valuable elements, the fragments of truth they hold. In short range, this has some drawbacks such as hostility and a success similar to that of Cassandra's predictions, as it results from the citation rate of my papers which are politely and systematically ignored [2-5].

"When truth is discovered by someone else, it loses something of its attractiveness" A. Solzhenitsyn

In the longer range, after the recognition of the theory, after its metamorphosis from an "ugly duckling" into a swan, hopefully laying golden eggs, it has to be adequately extended and developed, in order to cure its inborn mathematical debility. But now we need a theory which can be used as a solid working hypothesis and we have to follow one of the advises of the Father of Scientific Management:

"Quantification without adequate logic is worse than no quantification at all."
—Peter F. Drucker

In my opinion, we have to create first a very general and not precisely defined frame useful for the commercial development of new energy sources; real, profound understanding and impressive formulas will come later.

"In science the primary duty of the ideas is to be useful and interesting more than to be true" —Wilfred Totter

If we are pragmatic enough, we have to accept this, and if are not, it doesn't matter: the situation cannot be changed. However I am not fanatical about my opinion or my theory:

"When people are fanatically dedicated to political or religious faiths or other kinds of dogmas or goals, it's always because these dogmas or goals are in doubt"
—Robert Pirsig

And a general acceptance is neither possible nor desirable:

"It is time to understand that consensus in the scientific community is not a good sign, but rather a symptom of crisis. The polarization of views is normal" —V. Koliadin

To generate a viable mental tool, I have used both scientific data and basic principles of action and management: this is justified by the extreme difficulty and novelty of the problem [6].

"Problems worthy of attack, Show their worth by hitting back." —Piet Hein

The Perils of Neophobia (and Its Justification)

A very great part of the problems faced by Cold Fusion are due to the normal, self-protecting, neophobic reaction of the scientific community.

"All great truths begin as blasphemies"
—G.B. Shaw

"The mind likes a strange idea as little as the body likes a strange protein and resists it with similar energy. If we watch ourselves with honesty, we shall often find that we begin to argue against a new idea even before it has been completely stated"
—Wilfred Totter

"One of the marvels of creation is the infinite capacity of the human brain to withstand the introduction of knowledge" — Theodore Roosevelt

"When you don't understand something, you are against it"
—Grigore Moisil, Romanian mathematician

My dear Cold Fusion fellows, you have to understand that:

"What men really want is not knowledge but certainty"—Bertrand Russell

Cold Fusion was discovered by chemists, but the physicists are convinced that real or not, CF is their field and has to be treated their way. This generates some trouble as it can be concluded from an editorial entitled "Are there limits of scientific knowledge?" [7]

"..there is a difference in the way that physicists and chemists regard science, and I sincerely hope my physicist colleagues are not unduly offended. Many physicists hope they can understand EVERYTHING and they manage to pronounce this hope with great regularity to the public. There's a certain arrogance there, but also an ambition one must respect. Chemists seem to have no such problem. The complexity of molecular behavior- the origin, phase state, dynamic structure and, above all bond making and bond breaking reactions of molecules-amounts to a complexity equivalent to that of a decent sized universe. Yes, physicists, a universe. Chemists appreciate the enormity of molecular complexity very well and they regularly say, "We don't

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completely understand it, and it well be a long time before we do." Contemplating molecular complexity is a good mental exercise in humility. Chemists appear to be quite content with this well adjusted, honest attitude even though as scientists we are sometimes less well regarded for our admitted lack of complete understanding of our subject."

—Royce W. Murray

Whether cold fusion belongs to the physicists, to the chemists, to both, or demands new specialists is an open question. Any kind of combined solution is possible. Being a chemical engineer, my expertise in complexity includes elements of know-how and direct experience in hypersensitivity, chaotic behavior of real systems, and unpredictability. I know that complexity, chaos, change, and paradox cannot be solved, but have to be managed. It is time to recognize the immense difficulty of the field, as well as the impossibility of finding or guessing solutions by starting from a particular aspect or from wishful, beautiful, but unfounded analogies ('miniature hot fusion,' 'room temperature fusion,' 'piezonuclear processes,' etc.) It is time to accept with joy that complexity is our mode of existence:

"The World is constructed designedly in order to fulfill the interest of the scientist, being infinitely complex and perfectible, providing an endless and eternal field of thinking and action for him" —Y.H. Prum

It is time to understand that such a problem will get a solution only if we use adequate heuristic principles.[6,8].

The Way of Strategy

A strategy is essential because:

"If you don't know where you are going, any road will take you there"

—Theodore Levitt

My offer is a logical strategy based on the following five modes of confronting the problems:

Global Approach

Consider the entirety of the available data, information, knowledge, all the systems, results, isotopes of hydrogen. Only a cooperative combination between reduction and holism, analysis and synthesis, depth- and breadth-oriented methods will help us to achieve an integrative vision of the intricate problem: what is actually Cold Fusion?

Open Approach

Supply and use a continuous influx of information from the neighboring fields (no good fences!) and even from more remote areas of the human thinking and action. We are not permitted to forget that Koestler has proved that creativity works with 'bisociations,' i.e. associations between seemingly unrelated or even mutually alien concepts.[9]. And Goedel's theorem shows that it is never possible to solve all the problems of a system by using only *inner* data, information, and knowledge. Even a hasty investigation demonstrates that the neighboring fields of solid state science are lacking theoretical underpinning[10], and therefore an idea such as "cold fusion has no theory, therefore doesn't exist" or "only theory can support the advancement of CF" are completely false regardless of who issues them: a skeptic or an adept. The best method to stop progress in any science was stated by a skeptic, whose fervor, negative enthusiasm, and continuous combat against cold fusion has helped him to be quoted in a rather esoteric society of participants:

"Let's get the theory sorted out before we make claims about practicality"

—Alan M. Dunsmuir on the Internet, Fusion Digest No. 3103, January 1995

Focused Approach

Concentrate on the essential aspects, on the core of the problem. This core is *excess heat*, because cold fusion is oppressed and needs a quick final victory in order to flourish; there are many more chances that the savior of the field will be the Invincible Cold Fusion Demo— demonstrating that the energy source of the future has been found— rather than the Absolutely Convincing CF theory. Nuclear ashes obtained in small quantities in some of the CF systems have only scientific significance and it is more and more obvious that these are secondary products; 'Focused' and 'Global' are thus complementary.

Positive Approach

Accept both the positive and the negative results, that is, be positive toward both, and to make use of both. I have used the plethora of negative results to deduce the catalytic nature of cold fusion, i.e. I have used negative data to obtain positive, and possibly essential information. The extreme case of this approach is my claim to have founded Scipiology (the science of converting disasters in triumphs) [11]. Managers know well the importance of positive thinking:

"Here is one secret of success. Avoid being against some-thing.Instead be for *something. Examples. Instead of being against illiteracy, be* for *literacy and you will help to improve literacy. Instead of being against your company's policy, be* for *an improved policy. What happens: Whatever you are against, works against you. You begin fighting it and become a part of the problem. But when you state what are you for, you begin focusing on the potential for the positive change"* —Wayne W. Dyer[12].

Here I have to confess that it is quite difficult to apply this doctrine in one's attitude toward the militant, active skeptics. Perhaps one of my cold fusion friends is right when he states that 'a 99% skeptic is better than a 200% believer' but this is also open to interpretation. I have many good friends among the skeptics, however they have taught me that the next statement, which demonstrates the existence and power of the *negative* thinking, is not a metaphor:

"If you don't think at all, you think more than the average of the people."

—Jules Renard

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During writing this piece I came across an essay of Daniel Boorstin dedicated to the virtues of the so-called"negative discovery," i.e. paradigm changes which prove that "some long-admired fixture of the imagination does not exist"[13]. It is clear that cold fusion is also a "negative discovery" in the most positive interpretation of this wording. This shows once again its progressive character:

"Perhaps the modern realm of discovery is no longer a realm of answers but only of questions, which we are beginning to feel at home in and enjoy. Perhaps our modern discoverer is not a discoverer at all but rather a quester in an Age of Negative Discovery, where achievements are measured not in the finality of the answers, but in the fertility of the questions."
—Daniel Boorstin

Our case illustrates the conceptual richness of the positive-negative dichotomy.

Realistic Approach

Admit the limits of the present understanding and realize the present stage of development. The following advice is many thousand years old but very rarely understood:

"Gain power by accepting reality." — Chinese proverb

It isn't easy to accept that:

"Truth is never pure and rarely simple" —Oscar Wilde

"Knowledge is but a struggle for knowledge. And we are always equally far and equally near it." —Ramon Sender
"Science, at the bottom, is really anti-intellectual. It always distrusts pure reason and demands the production of the objective fact" —H.L.Mencken

"Truth is perfectible at the most, only lies can be perfect."—Y. H. Prum

Or the worst of all:

" Theories are ways of thinking which determine the choice of the experiment" —Mary Migley

Theory and know-how have to be in equilibrium because:

"When the cobbler became a shoe specialist, comfortable shoes could no longer be found" —Erwin Chargaff

We can learn from the great managers; a single quote is convincing:

"Past wisdom must not be a constraint but something to be challenged. Yesterday's success formula is often today's obsolete dogma. My challenge is to have [the organization] continually questioning the past so we can renew ourselves every day."—
Yoshio Maruta, chairman of Kao

Skeptics and cold fusioneers unable to think beyond the basic Pd/D2O system, please take note!

The Light at the End of the Tunnel

"I hate quotations! Tell me what do you know!"—B.Disraeli

O.K., Ben, you are right, no more quotations, and I'll tell you because I am looking back with pleasure. However, I am quite busy looking forward.

Cold Fusion Systems

After more than 25 years practice in the systematization of patents and elements of know-how for chemical technologies, taxonomy is my baby. I have learned how to classify —chronologically, in order to comprehend the evolution —logically, i.e. according to the kind of the solution, trying to get a global vision of the possibilities —technologically, that is by the companies owing the patents, correlating the data aiming to a realistic view of the technologies worked out by the leading companies. For my specialty (suspension polyvinyl chloride) I had processed more than 12,500 patents and, obviously, had no computer. Then I joined the Cold Fusion movement (actually it happened a few years before the F & P press conference; hot fusion seemed to contradict my theory of unstoppable technological progress and I *knew* that an alternative solution must exist) and when very soon the great stumbling block of irreproducibility came in sight, I immediately identified the stigma of catalytic processes. To be catalytic is a blessing in the case of a well-tempered process, but is a curse when it's beyond control. All CF systems discovered are cases of catalysis [2-4] and using this criterion they can be classified in the following logical way, using as criteria the location of the active sites and the method used for raising them:

1. WET SYSTEMS (gas/liquid/solid interfaces)

1.1. Electrochemical:
- 1.1.1. Pd/D_2O
- 1.1.2. Ni/H_2O
- 1.1.3. Pd/ molten salts

1.2. Ultrasonic:
- 1.2.1. Pd/ D_2O
- 1.2.2. Metal/H_2O

2. DRY SYSTEMS (gas/solid interfaces)
- 2.1. gas loading/unloading
- 2.2. gas discharge
- 2.3. gas sparking
- 2.4. gas/solid, stimulated
- 2.5. gas/ proton conductors
- 2.6. ionic implantation

Only systems in which excess heat was obtained are included here. Who knows, there may be others! Details regarding these systems can be found in the reviews of Storms[14,15] and hundreds of other papers and patents.

Occam's Razor

The systems are very diversified, and it seems that what they have in common is the *inhibition* of the positive effects and the *immediateness* of the negative effects; excess heat generation can be triggered with great difficulty but can be easily interrupted. It is obvious that the cold fusion effect is based on some entities which are very difficult to breed, but easy to destroy. In other words, they are hypersensitive and determine a chaotic behavior of the systems if these are in a sub-optimal condition. However there are

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striking differences regarding the practical means of managing the different CF systems. Storms calls the 'entities' a Special Condition of Matter (SCM) and states that this condition can appear in different chemical environments. He states: " The challenge for a theoretician is to find what these SCM's have in common." In my opinion all the CF systems described are based on methods of activation: electrochemical, ultrasonic, and others specific for gas/solid interfaces, that is, on creation of catalytic *active sites*. As seen from the practice, both in the chemical industry and in case of cold fusion, this isn't an easy job. Working by cavitation, the process is quite powerful and unperturbed, in contrast with the "classical"Fleischmann-Pons cell where the active centers are created long after all diffusional processes have attained a state of equilibrium. This suggests another criterion, yet not well defined, for ordering the CF systems from the most tough and efficient to the most sluggish and delicate. Catalysis is inherently economical given the processes are very *localized* and only an extremely small fraction of the matter has to be in the productive "Special Condition.' And this condition isn't bound to a special composition but it is actually a quantum state determined by the topology and the dynamics of the atoms placed in the active sites. For this reason, I am using the name "surfdyn concept" for my working hypothesis. Quantum confinement, quantum corrals, and quantum cavities are the probable scene for the unexpected processes. (I don't like the wording "anomalous," the anomalous of today is the trivial of tomorrow.)

Isotopic Democracy

The participants in the Cold Fusion drama are as surprising as the scene. At the very beginning of the story it seemed that only deuterium could play a role, and D+D fusion was the name of the game. Now we know that in at least 5 systems (1.1.2., 1.2.2., 2.1., 2.3., and 2.4.), light water (or hydrogen) has an excess heat producing potential similar to that of deuterium. A recent "surprise" the very first bastion of deuterium has fallen: using palladium-coated beads with very high surface area (that is with enhanced catalytic activity), Patterson [16], was able to obtain excess heat with a H_2O-based electrolyte. Reifenschweiler [17] has published data about the temperature dependence of the radioactivity of tritium embedded in titanium soot, having nanometric particles. The myth of inaccessibility of the nucleus by low energy processes has fallen. Where does this miracle happen? In a catalytic environment, of course! With another isotope of hydrogen. As far we know, there are no essential differences between deuterium and protium regarding the level of excess heat. A kind of Isotopic Democracy is working, possibly in all the systems. However, this proves that Cold Fusion has a component which is not nuclear. Who cares as long as it is a reliable source of energy?

The Cold Fusion Scenario

A simple scenario, hiding a treasure of complexity can thus be imagined:

"Cold Fusion" is actually a combination between: a Catalytic Quantum Effect, providing the capture of the zero-point energy, and more Catalytic Nuclear Effects, leading to nuclear ashes.

Obviously the nuclear particles are able to induce secondary reactions. The quantum non-nuclear and the nuclear effects have different ratios in the systems described and this is a kind of mark of each system. The nuclear contribution to the excess heat is significant only in the deuterium based systems when helium-4 is formed; the maximum "nuclearity" appears in the palladium-heavy water ultrasonic system. According to my guess, even the Fleischmann and Pons cell is only 20-25% nuclear. (Yes, my reputation is at stake!) Eventually let's imagine a very simple play, based on the concept of pairing postulated by Reifenschweiler. Two atoms of hydrogen isotopes, confined in a quantum well, can form a pair (lots of theories describe aspects and forms of this idea). Beyond a degree of pairing they are able to tap ZPE, and excess heat is emitted. At higher degrees of pairing and if deuterium is present, the nuclei are fused and a new source of heat is at work. An analogy can be found: the nuclear events are like marriages, while the quantum events are like pairings, both intra- and extramarital. This gives an idea of the relative frequency. Obviously, the real situation, (of cold fusion) is much more complicated. I don't know when my theory will be accepted. In the worst case the final victory will be semantic: the catalysis jargon will invade and conquer the field.

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The Energy Crisis

by "Tom Flame"

(With apologies to Tom Paine, American revolutionary, 1737-1809)

Now is the time to change our goals. Petroleum potentates and certain self-satisfied executives will, in this time of crisis, shrink from true service to humanity. But he who stands up now deserves the love and thanks of people everywhere.

Our addiction to fossil fuel, like any narcotic, is not easily broken. Yet we have this consolation, that the harder the struggle, the more glorious the triumph. What we obtain too cheaply, we rate too lightly. It is scarcity alone that gives everything its value and Nature will hide the truth, if she can. So it would be strange, indeed, if so precious an article as free, non-polluting energy should not be very well-hidden.

I call not upon a few, but upon all: engineers, scientists, investors, and bureaucrats too—jump in and help us! Lay your capable shoulders to the wheel. Better to have too much force than too little, when so great an object is at stake. Let it be told to a future world that 'neath this pall of smog, where little but despair and indulgence could be found, that the thinker and the tinker, alarmed at a common danger, labored day and night to commercialize "cold fusion."

It matters not where you live, or what your station in life, or the evils of our profligate consumption— the blessings of new hydrogen energy will reach you all. The far and the near, the rich countries and the poor, the industrial, transportation, and residential sectors of our economy, too; all will suffer— or be blessed with renewal.

The person who feels not now is as good as dead. The plaintive wheezing of his asthmatic children will be a curse to he who shrinks back at a time when a little might have saved the world. I love the man (and woman, too) who can smile in trouble, gather strength from the deprecation of tyrants, and grow brave in reflection. 'Tis the custom of little minds to shrink from impossible challenge. But he whose heart is firm, and whose conscience approves his conduct, will fight on and on until victory is achieved.

There are cases which cannot be overdone by language, and this is one of them. There are persons, too, who refuse to see the full extent of the evil which threatens them. They find solace with the hope that oil magnates even now are dutifully seeking a solution to pollution. It is madness or folly to expect zero-emissions technology from those who selfishly encourage, and profit so greatly from, our foolish dependance on carbon-based fuels.

But I see no real cause for fear. I know our situation well and can see the way out. With perseverance, modest investment, and practical "cold fusion" engines, a glorious future lies ahead. By cowardice and resigned submission to the status quo, we would have nought but the sad choice of many evils—a ravaged country, depopulated cites, habitations without safety, and air we cannot breathe. My countrymen *will* make the only right choice!

Adapted by Geoff Rohde of Davis, California, from the original, inspiring words of Tom Paine, America's foremost "motivational speaker," writer, and rabble-rouser.

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A Warm Welcome to *INFINITE ENERGY*

by Hal Fox, Associate Editor

The friendship began in July 1989, when I received a call from MIT. "This is a seeker of truth, what's going on in Utah with cold fusion?" That call was from Dr. Eugene Mallove and a friendship was started. I'm always partial to **seekers of truth**.

Yesterday, I talked to a script writer from Canada, who is working out the details for a new stage play on cold fusion. He had two source books—one was *Fire From Ice* by Eugene Mallove, the other was *Bad Science* by Gary Taubes. After listening to his comments for two minutes, I found that this script writer had no difficulty in separating fact from fiction. He commented that the Taubes book was so blatantly negative that he knew it could not be factual. My suggestion was that he had *Fire From Ice*, the best book yet published about cold fusion and that he could depend on the factual presentation by an excellent researcher and writer.

Eugene Mallove has not only written the best history of cold fusion, he has been strongly instrumental in helping change the sorry history of denial of cold fusion by the establishment, through his activities with Jed Rothwell in Cold Fusion Research Advocates. With this umbrella, Gene and Jed provided every member of the U.S. Senate and House of Representatives with both video and written information about cold fusion. Today, that type of spreading-the- word is bearing fruit in the growing disgust with the actions of the Department of Energy and its "pathological skeptics," which has resulted in the U.S. largely abandoning its position in cold fusion. Now the new Office of Technology Assessment will be staffed by advisors competent to recognize new science. The Department of Energy, which should have been the world's foremost developer of new energy science, is hiding in its own shadow of incompetence.

The road from being an early "searcher for truth" to the editor and publisher of *Infinite Energy* has been challenging, and not financially rewarding. There are few who have the intestinal fortitude to quit a job with one of the nation's leading universities rather than be used as a purveyor of an abortive attempt to bury cold fusion. The editor-in-chief of *Infinite Energy* is a man of high principle and I am pleased to join his staff in this endeavor to provide another growing voice for you who are also **seekers of truth**.

This is a monumental time to usher in this new publication. On the eve of the Fifth International Conference on Cold Fusion, there are dramatic cold fusion achievements that are now being commercialized. In addition, there are equally dramatic new developments in other enhanced energy systems. Solid-state circuits are now producing a reported ten times more energy out than input. Water cavitation devices are being manufactured and sold that range from 110 to 300 percent efficiency. New magnetic motors and generators are running "over unity." *Infinite Energy* is here in more ways than one. Your life is about to be changed. ∞

***Infinite Energy* is here in more ways than one. Your life is about to be changed.**

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Reprinted from *The Peterborough Transcript*, New Hampshire, February 9 , 1995:

Andrew Rothovius views the universe

The Dawning of the New Energy Age

Though it may well have escaped your attention, what with the daily overdose of O.J. Simpson and Newt Gingrich that the media continue to heap on us, there have been several other happenings of more than just momentary importance during the first month of the year.

In the natural world, the Pacific Ocean warm-up ("ElNiño") was on a winter rampage for the third time within the past six years—an unprecedented frequency—knocking long-range weather forecasts down like ninepins right and left, and flooding vast areas from California to Holland.

The volcanic and seismic "Ring of Fire" around that same Pacific Ocean was in a renewed spate of activity, knocking down a city in Japan, shaking up Seattle suburbanites, and emitting an ashy plume from Mexico's Popocatepetl, which sits like a volcanic sword of Damocles over Mexico City and its nine million inhabitants, who could soon be faced with a disaster even worse than the recent collapse of their currency.

In the human world, we had the extraordinary discovery within a few weeks of each other of the finest prehistoric rock paintings yet found anywhere, in a French cave and on a Portuguese cliffside.

And a pair of Greek archaeologists claimed to have found evidence that the long-lost tomb of Alexander the Great is in the Siwa oasis in the Libyan desert, far from anyplace where it had previously been sought.Any one of those events could easily have been the subject of today's column, and some may yet be that of future ones. Today, however, I want to update my readers on the ongoing cold fusion story, which though a late entrant still rates a high chance of being the greatest of this century—or perhaps of any century since history first began.It may be recalled that I have taken a look at cold

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fusion a few times already, at intervals of roughly a year or so, since it first became a subject of media attention, and mainly ridicule, some years back.

About once a year is also the spacing of the *Wall Street Journal's* occasional grudging admissions that there is actually something of importance in the growing development of cold fusion. Their latest column on it by Jerry Bishop, who is an able and unprejudiced science writer, but is kept on a tight leash by the editorial board, appeared almost eight months ago, on July 14.

The last time I touched the topic here was in November 1993, more than a year ago, so I am a bit late in getting back to it—and there's a lot to report on what has been taking place in the interval.

On Saturday, Jan. 21, MIT hosted at its physics lecture hall in Cambridge, Mass., a presentation and panel discussion by some of the most active cold fusion researchers and developers.

While.this meeting—that will, I predict, be someday looked back on as the signpost of the coming transition from one era to another— was largely ignored by the media, its implications are clear: the New Energy Age is almost here, and its advent will catch an awful lot of people by surprise.

Prototype cold fusion reactors for running automobiles are probably now only a year or two away. So are the first prototype household generator units. Within five years or so—as the new century begins— such units should be commercially available.

They will be expensive at first, probably around $30,000. Even at that price, they will be a good investment, for they will run every appliance in the house, and heat and cool it, on zero fuel consumption, since they will run on minute amounts of heavy water.

It will not take long for increased volume and manufacturing simplification to bring the price down into the $3,000 ballpark, or even lower.The furthest advances in the field are currently being made by Prof. F. Piantelli of the University of Siena in Italy, who is being funded by the Fiat Motor Company, which hopes to get a head start on its competitors in applying cold fusion to automobile propulsion.

Last year, Piantelli was getting a 60 watt sustained energy output from his cold fusion cells. He is now developing a 1,000-watt cell and hopes to upgrade to 10,000 watts, enough to power a test model Fiat car.

At that point, General Motors and Exxon and OPEC may all start to get the shivers. Automakers who do not convert to the New Energy propulsion will eventually be driven out of business. So, in process of time, will petroleum producers.

Not overnight, of course. It took two or three decades for the automobile to fully replace the horse.

Conversion of the buying public could be speeded up if current experiments, such as those being carried out by Hydrocatalysis Power Corp. of Lancaster, Pennsylvania, confirm that cold fusion cells yield adequate power with ordinary water and nickel cathodes, instead of the more expensive heavy water and palladium that have been mainly used by other developers.

"New Energy" is the term coming increasingly into use to denote the revolutionary new forms of energy production that cold fusion is spearheading.

As it becomes ever clearer that something other than what current nuclear and chemical function theory can explain is involved in the cold fusion phenomenon, researchers are finding that energy can also be produced in other novel and so far inexplicable ways, that may—or may not—be related to cold fusion.

One of the most exciting and potentially far-reaching of these spinoffs, if that is what they are, is the magnetic motor being developed by Bertil Werjefelt of Hawaii-based PolyTech (USA).

He reported at the MIT meeting that he is getting three times more energy output than input, in his test models, and expects shortly to have a self-sustaining model in operation.

Independently of Werjefelt, though communicating with him, four major Japanese firms— Sumitomo, Hitachi, Mitsubishi and Matsushita—are pursuing the same line of investigation, aiming at a commercial version within the next few years.

Such a magnetic motor—in cars and appliances—would cut energy usage by enormous quantities, even before cold fusion comes into widespread use.

Then there is the Hydrosonic Pump developed by James Griggs of Hydro Dynamics, Inc. in Rome, Georgia. This delivers fantastic amounts of excess energy through what Griggs believes is an unconventional nuclear fusion reaction, though there is yet no clear explanation of just what process is involved.

In any case, the pumps work, yielding up to 250 horsepower at 10- 50 percent [excess power production]; and Griggs is already selling hundreds of thousands of dollars' worth of them a month to satisfied customers in textile and other industries where such pumps have a thermal application.

This is much to the dismay of the skeptics, who had been labeling Griggs as incompetent, ignorant, dishonest and greedy.- ENECO, a Salt Lake City-based firm that has acquired a commanding basic patents position in the field, is working on prototype units of a nature yet to be disclosed. Clean Energy, Inc. of Dallas, Texas, has been granted the first specific cold fusion patent, for a unique palladium-coated spheres system.

Several other forms of unconventional energy production are being experimented with, in various forms. They include a revival of the far-out proposal of Dr. Robert L. Carroll, now 85, which he first floated way back in 1971, for a very cold fusion of platinum and hydrogen, at close to absolute zero.

Whether or not such extremely exotic ideas ever get translated into reality is beside the point. What matters is that the emergence of cold fusion is opening up a vastly fertile new era of invention and scientific advance, which in a relatively short time will transform the conditions of human existence more radically than did the advent of steam, the gasoline engine, radio and TV, and aircraft.

A century ago this coming November, Wilhelm Roentgen announced his discovery of x-rays, a form of radiation. This was the seminal starting point from which the world of that time was transformed within the span of less than a generation.

People living in 1895 had no idea of airplanes or radio or radioactivity or of relativity or motion pictures. If they had heard of automobiles, it was only as an expensive toy of the rich.

Yet those who lived another 20 years would see all these having become familiar objects or topics. Ten years further on, they would be an integral part of the common fabric and background of daily life. We live today on the threshold of an even greater era of radical change. It will be resisted by many, but they cannot hold it back for long.

If the electronic media give anything like adequate coverage to the Fifth International Conference on Cold Fusion, to be held in Monte Carlo, Monaco, April 9-13, a more general realization of what is coming will spread rapidly across the globe.

If the media persist in ignoring the really biggest story in today's world, the public will eventually learn of it when they start to see the commercials for cold fusion-powered autos and household energy sources.

In the meantime, I suggest sending $5 to Cold Fusion Technology, Box 2816, Concord, NH 033022816, to receive a report of the latest research results. ■

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A "Micro-fusion" Reactor:

Nuclear reactions in "the cold" by ultrasonic cavitation

by Tom Benson

With a compact device now being developed into a major tool to study micro-fusion "cold fusion" phenomena, laboratory researchers can generate excess heat and helium-4, and they can vaporize palladium foil under heavy water. Is this "cold fusion"? Whatever it is, it's hot, and it's fascinating.

"Burning" palladium foil under water

Consider a challenge: Begin with a small square of palladium metal foil, which has a melting temperature of about 1,550 °C. Immerse the foil in cool water. Then, using only about 100 watts of power incident over the area of the foil (such as the power consumed by a standard light bulb), find a way to melt a hole in the metal in less than five minutes.

Inventor Roger Stringham and Russ George of E-Quest Sciences, Inc. of Palo Alto, California use this to illustrate the unusual ability of their new laboratory research tool. Their ultrasonic cavitation "micro-fusion" device *can* melt palladium foil, by delivering only 100 watts of power in ultrasonic waves impinging on the foil under heavy water. The ultrasonic waves create cavitation bubbles that drive what Stringham and George firmly believe to be a new type of nuclear reaction inside the metal. This vaporizes the foil material and appears to release energy that many observers believe to be from new kinds of nuclear reactions —nuclear reactions "in the cold." Moreover, the under-water melting can occur within minutes of start-up. It can be reproduced essentially every time that it is tried—100 percent reproducibility.

Stringham and George are fond of showing scientific colleagues a melted sample or even a videotape of the melting process. They say that their heat-measuring calorimetry is proof that their device produces excess energy. They also claim to have found a variety of unique nuclear products—most notably helium-4 and helium-3— from the reactions induced in the experiment. They proudly show a scanning electron microscope (SEM) photo of the surface of the affected metal. It reveals a landscape pock-marked by a landscape reminiscent of extinct "volcanos" of melted and re-cooled metal. This is unambiguous evidence, they say, of microscopic fusion-like explosions taking place deep within the metal. Doing some relatively simple mathematical calculation on the energy required to melt and eject the observed volume of metal, they note that no form of localized *chemical* energy could possibly produce these effects.

The E-Quest micro-fusion process has also been demonstrated at Los Alamos National Laboratory and at SRI International. It has also been offered for sale as a working experimental device by the young California company. The E-Quest team, armed with several years of carefully researched technology, may well be on their way to a major goal: a safe, controlled form of nuclear power, with little or no radiation, and with the potential for near-term development into larger products.

How does it work?

The E-Quest process uses ultrasonic cavitation to "turbo-charge" "cold fusion." The research of the past five years suggests that the trick of successful electrochemical "cold fusion" was to load large amounts of heavy hydrogen nuclei—deuterons— into the sponge-like matrix of palladium. There, the artificially high pressures and unusual conditions would presumably induce unusual fusion or other exotic nuclear reactions. The difficulty of cold fusion has been to find ways to reliably pack enough deuterons (D) into the metal to achieve a sufficiently high loading factor—the ratio of D to Pd atoms, D/Pd—to trigger the reactions. If the palladium metal conditions (e.g. having a crack-free metal sample) are not just right, the electrochemical cold fusion effects will not appear. That may be why so many groups in 1989 said they failed to reproduce the "cold fusion" results. It was extraordinarily difficult to attain the requisite loading level, above D/Pd = 0.85. Traditional techniques—especially electrochemical loading—were not ordinarily suited for the job, except in the hands of experienced electrochemists.

By contrast, the ultrasonic cavitation method more readily produces reliable results. In this process, the palladium, titanium, or other metal target is immersed in heavy water (D_2O) and the target is bombarded with high-intensity sound waves. The high-frequency sound waves (typically 20 kilo-Hz) cause the expansion and contraction of tiny, so-called cavitation bubbles. The pressures and temperatures inside the bubbles, as they are compressed, can be very high. Temperature estimates range into thousands or even millions of degrees, which has kept researchers in "sono-chemistry" fascinated for years.

In the E-Quest process, these compressing bubbles collapse close to or on the palladium surface. Instead of collapsing and disappearing from the fluid, the bubbles actually collapse *into* the metal surface. Photomicrographs show that the bubbles form a funnel-shaped jet, which injects the contents of the bubble (now, in part, a deuterium plasma at extraordinarily high temperatures and pressures) into the metal lattice.

The advantage of the ultrasonic techniques is that it is highly effective in rapidly packing deuterons into the lattice of metal atoms. With bubbles being formed and compressed at the acoustic vibration frequency of 20 thousand cycles per second, there are huge numbers of bubbles that impact the surface; each bubble has billions of deuterons in its volume. This means that the cold fusion electrochemical loading "trick," which before was hovering at the edge of impossibility, is now very easy to bring about. It is possible to initiate the micro-fusion reaction in a matter of minutes instead of hours, with nearly 100 percent reproducibility.

The Mark II

One of the E-Quest designs, the Mark II, looks deceptively simple. The entire unit occupies only several cubic feet of space, but the guts of the reactor is much smaller. Its core is a hollow cylindrical chamber, a few inches in diameter and about 1/4 -inch thick, fashioned from machined stainless steel—like a thin slice of a soup can. At one end of this chamber is an ultrasonic generator—an electromagnetic or a piezoelectric vibrator, which is powered by an external electrical source and which vibrates at about 20 kHz. The vibrating "plate" forms the end-cap of the chamber, which is filled with heavy water (D_2O). The core chamber can be pressurized with a gas such as D_2 or argon, and it normally is pressurized when the system is being operated. The heavy water is recirculated in a closed-loop through the core—entering through an input port and exiting through an output port and an external heat exchanger. Inside the core and immersed in heavy water, opposite

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the vibrating plate, is the metal target—a thin sheet of metal (typically 0.1 mm thick and about 25 cm^2).

The temperature of the cooling water as it enters and exits the core is monitored by thermocouples. The temperature of the heat exchanger for the cooling water and the interior of the core are also monitored for temperature with thermocouples. From these temperature measurements, the flow rate of the water and from the measurement of the input power, it is possible to compute the power going into the system, as well as the power being produced by the system in the form of heat. Some of this heat, of course, is the ordinary and unremarkable heating due to dissipation of the acoustic energy in the water and on metal surfaces. But what the calorimetry also shows, when the unusual reactions are occurring, is an *excess* power that cannot be explained by ordinary dissipation of acoustic energy.

The ultrasonic transducer creates high intensity acoustic waves in the heavy water. As these ultrasonic sound waves pass through the liquid, providing that appropriate pressure and temperatures are maintained, bubbles form in the liquid. A bubble spontaneously pops into existence when the "low pressure" part of the sound wave passes. Then the bubble is compressed back down when the "high pressure" part of the sound wave passes. Thousands of these tiny bubbles are created in the fluid, and the cycle repeats itself 20 thousand times per second. In a matter of minutes, literally billions of bubbles live out their short lives in the reactor core.

Cavitation bubbles as energy focusers

What is it about these bubbles that produces such an extraordinary effect? They are certainly nothing new; even a century ago, cavitation bubbles created by the action of the propellor blades were known. The physics and mathematics of bubble formation and collapse were first described in 1917 by Rayleigh. In recent years, there has been a flurry of interest in the cavitation process, especially in the effect known as *sonoluminescence*, the emission of blue light from cavitation bubbles. Sonoluminescence and cavitation have been discussed extensively in scientific journals and popular science magazines such as *Scientific American*. These works explore the characteristics of ultrasonic cavitation and its its application to the traditional areas of physics and chemistry.

In the case of cavitation-induced fusion, however, E-Quest is exploiting another interesting characteristic of the bubbles: their ability to focus or concentrate energy into tiny high energy volumes with very short lifetimes. This is used to drive the micro-fusion events. How?

The secret is the way the bubbles form and collapse. During one acoustic cycle—a sine wave— there is a low and high pressure region. Bubbles are created and grow in the low pressure region and collapse in the high pressure region of the acoustic cycle. That sounds simple, but there are other factors: the expansion and compression of the bubble are radically different. In the low pressure region, the bubble grows relatively slowly and steadily, continuing its growth from sheer momentum even after the acoustic pressure has become positive. This phase of the bubble's expansion is *isothermal*, which means that the bubble grows slowly enough that it stays at a temperature approximately equal to its surroundings, even as it gains in size and mass. This means that the total energy in the bubble is growing—it is removing energy and materials from the surrounding water.

At a certain point, the sound wave pressure finally becomes high enough to reverse the growth of the bubble; the bubble finally "turns over" and begins to collapse, but this doesn't happen slowly and smoothly. Because of the lag in time to begin collapse, which is due to the momentum of the bubble expansion, the bubble collapses very fast —within approximately 1% of the total cycle time! And it may collapse up to one- millionth of its largest size. This occurs so quickly that, unlike the expansion phase, the collapse happens *adiabatically*, which means that the bubble does not have time to equalize temperature with it's surroundings. During collapse, it does not give up the energy it collected during expansion. The energy and vapor in the bubble are trapped and nearly instantaneously compressed into a tiny, intensely hot region.

Something quite interesting happens to collapsing bubbles that happen to be near the solid surface of the target. If a bubble is simply suspended in water, it collapses and disappears. If it is next to a metal surface, it does something quite different. At the final, catastrophic moment of collapse, it forms a funnel-shaped jet—which injects the contents of the bubble into the metal.

During this high energy collapse, the contents of the bubble, formed from the vapors of the liquid, are compressed and superheated to a plasma-like state. In that final instant, the material in the bubble-jet may be in a state of confined matter like that found in stars—perhaps reaching temperatures of millions of degrees, pressures in the millions of atmospheres, and densities greater than lead. It is this extraordinary state of matter that is injected into the metal.

The key to the ultrasonic cavitation system, therefore, is the concentration and delivery of energy and material. The acoustic wave provides the raw energy, the bubble is the method of concentration and focus, and the jet is the aiming device that plunges this high energy plasma into the metal.

Double Bubble Trouble

The precise process by which micro-fusion events happen is not well understood. However, given the calculated pressures, temperatures, and conditions at the microscopic scale, we can make some reasonable guesses.

In electrochemical "cold fusion," results depend on loading metal with deuterium atoms. The purpose of loading is to cram as many of the deuterium atoms or nuclei as possible into the spaces between the metal atoms. Some cold fusion theoreticians speculate that the fusion reaction happens when a large enough number of these atoms are loaded into the metal lattice.

In ultrasonic cold fusion, the metallic matrix is loaded with deuterium, a long-term build up occurs from the numerous bubble-jet impacts on the metal. The number of bubbles produced on the metal surface (25 square centimeters) is on the order of thousands during each acoustic cycle, meaning that tens of millions are produced there each second. Moreover, the metal lattice of atoms is under huge stress from the acoustic field in the metal and from shock waves generated in the collapsing bubbles; it is vibrating and pulsing with chaotic energy stimulation.

Consider this scenario: A bubble-jet injects a mass of high-density, high- temperature deuterium atoms, at high velocity, straight into a spot in the metal matrix. This area is already crowded with other atoms, which do not have enough time to move out of the way. For an instant, a condition of extraordinarily high pressure exists for the deuterium atoms, trapped within the lattice of palladium atoms. In that instant, a nuclear reaction for some type—possibly fusion— occurs.

There are many unknowns about this final event; for example, the temperature of the bubble. Clearly, what is *not* happening is ordinary *hot* fusion. If it were, the experimenters operating the Mark II would be killed by a blast of lethal neutron and gamma radiation. Certainly not all of the bubbles cause nuclear reactions; if that were the case, the entire metal would

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vaporize instantly, from the huge energy released. However, some small percentage of these bubble events are causing localized reactions—energetic enough to leave the formerly smooth, featureless metal surface looking like a volcanic landscape. The metal appears literally to have boiled and formed eruptions in "volcanic"-like fashion, leaving behind craters and molten material.

Results of some experiments

After an experimenter using the Mark II turns on the pumps and sets the pressure of the "cover" gas, he or she switches on the power supply to the the ultrasonic transducer—the sonicator. Immediately, a screeching high-pitched sound is heard, signifying that the sonicator is operating. Within a few seconds, the temperature of the cooling water begins to rise. If the system is run as a blank or a control—with ordinary water instead of heavy water, or with a stainless steel target instead of palladium or titanium, but still using heavy water— the temperature elevates smoothly, indicating that the ultrasonic energy is being absorbed and is heating the cooling water. However, when the system is run with heavy water and the appropriate metal, the temperature immediately (within minutes) begins to climb upward above the normal curves of temperature rise.

When palladium or titanium foils are used as targets, the effect occurs almost *every* time and begins within a few minutes at most, assuming that the experimenter has appropriately set the fluid pressure and temperature and other parameters. There is some "artistry" in getting this kind of experiment to work and the cavitation process working correctly. The pressure, temperature, and sound intensity of the system have to be set precisely, or no cavitation and obviously no reaction occurs. E-Quest scientists have learned to "tune" the temperature, pressure, and sound to get results within seconds.

The Mark II device produces a host of intriguing results. Taken individually, these results might be mere laboratory curiosities. Taken together, however, they seem to point to a nuclear process of heretofore unknown character. For example:

• Excess heat

The system produces substantial amounts of excess heat. As a percentage of the total input power of the system, the excess has been measured up to 80% of the input power. In absolute terms, excess powers up to about 100 watts have been observed. In calculating excess power, the inefficiency losses of the ultrasonic generator must be considered. For example: in one configuration, out of a total of 375 watts of electrical input power to the transducer, only about 100 watts actually appears as acoustic energy in the reaction chamber. To be on the safe side, the experimental results charts indicate both the excess power verses total power in, and excess power verses total energy reduced by the efficiency losses of the ultrasonic transducer.

No matter how the calculation is performed, the excess energy output is much higher than can be explained by any chemical reaction of the heavy water with the palladium or titanium. The excess heat output may, in fact, be understated. As Roger Stringham says: "We have never really tuned the device to maximize gross excess heat production, we are mainly looking for scientific data on byproducts and reactions. We are going to try a few modifications and see what happens."

• Helium production

These reactions produce significant amounts of helium-4, the most abundant isotopic form of helium. Helium-4 is present in natural air in only small amounts—typically about 6 ppm (parts per million). It is one possible form of the "nuclear ash" that so many critical scientists have asked cold fusion researchers to find. The helium-4 level has been measured in samples of the gas used to pressurize the Mark II reaction chamber. Using a mass spectrometer device at two independent laboratories, the concentrations of helium gas were found to be in one instance 65 ppm, but at least 10 times the pre-existing background level in the gas before the experiment was started. Scientists looking at helium-4 results are often concerned with leakage from the outside air. However, this is not a consideration, since these concentrations are far larger even than the concentrations in the outside air. In a series of experiments at Los Alamos National Laboratory in 1994, some exceedingly high levels of helium-4 were achieved. The sample gas was tested at Rockwell International, known for its quality helium measurements, and was found to contain 552 ppm helium-4— about 100 times background concentration in ambient air and 1,000 times the concentration in the argon cover gas.

• Extreme deformation of target

The most dramatic evidence for unusual reactions are the lingering effects on the metal foil targets. The foils exhibit gross evidence: a "burned" appearance, melted spots, and deformation. Microscopic examination is even more impressive. Photos of the metal, taken with a scanning electron microscope (courtesy Dr. John Dash of Portland State University in Oregon) show a metal surface that has been blasted from within as though it were a volcanic landscape. Metallurgists who have examined these SEM photos say that the metal has actually been vaporized—heated so intensely that it has evidently boiled off. If so, one would expect to find solidified particles of metal in the cooling water, and this, in fact, is the case: after running for some time, the cooling water is suffused with tiny round globules of melted and re-solidified metal.

Russ George notes. "This evidence is especially useful because it is frozen in time; it can be examined and investigated by scientists long after the experimental run is over.

• Control experiments are null

In every case so far observed, there are substantial differences in results between control experiments and active ones. Systems using ordinary water or non-reactive metals do not lead to destructive effects on the metal. They produce no excess heat that could be counted significant. Control runs have evidenced no helium-4 above background level. Since heavy water and light water are chemically identical, the fact that the heavy water system produces impressive phenomena and light water does not, shows that the effect is not chemical.

A bit of history: From the redwoods to Los Alamos

When Roger Stringham first thought of applying ultrasonic technology to cold fusion, he had been exploring uncharted territory for years. Following a productive career in such prestigious research firms as SRI (Stanford Research Institute, now SRI International) and SAIC (Scientific Applications, Inc.), he chose to become an independent scientist, using his home as a laboratory amid the redwood groves overlooking Stanford University. In his small lab, he studied a technology he found especially fascinating: Photo-sonication, which is a method of using the energy concentration effect of cavitation bubbles, combined with high energy light, to drive photo-chemical reactions that were impossible under normal circumstances. Although well within the boundaries of classical chemistry, it was quite an unusual technique; Stringham was one

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the few researchers who had figured out how to harness cavitation bubbles for practical applications.

When Drs. Pons and Fleischmann made their announcement on March 23, 1989, Stringham immediately thought of another application for his work. He says, "I had the idea, almost immediately, that the techniques I had already developed could be applied to this new field. The photo-sonication technology would be fairly easy to transfer, and could possibly produce some interesting results."

Within a few days of the announcement, he had adapted one of his devices to "cold fusion" and performed a series of experiments. Although his measuring techniques were simple at first, this first device produced completely unexpected and unique results when using the special "cold fusion" materials — heavy water and palladium. These effects did *not* occur when palladium was used with ordinary water. He also observed a tantalizing effect: "Almost in the beginning," says Stringham, "I had melted a small piece of palladium, which was a bit of a shock—I knew very well that that should not be possible. So I tried a much larger piece and melted it too. At that point.....well, you could say I was hooked."

Over the next few years, he worked diligently on developing variations of his technique: new apparatus, different reaction materials and gasses, and different configurations of target metals and geometries. For his own peace of mind (and that of his wife), he used health physics radiation monitors to detect possible emanations from the device, but he found no evidence of any harmful radiation. After struggling for some time up a "learning curve," he came back to his simplest and most successful design, which had produced the best scientific data and showed the most promise for further development.

Also, he had come to recognize the challenge of commercializing the technology; one does not easily come out with results that may help to shift the world's entire energy economy! So to begin telling the world about the discovery Stringham formed E-Quest Sciences in 1993 and gathered a team of old friends and new associates to help: Dick Raymond to run the business, Attorney Larry Klein, former mayor of Palo Alto and partner in a prestigious Silicon Valley high-tech law firm, to handle legal matters, and Russ George, a well-connected cold fusion scientist.

E-Quest strategies

The company decided to focus primarily on developing its technology, and working with other researchers. The company first filed carefully worded patents. At a very early date comprehensive patents were filed, supported by proof of both energy and nuclear product-producing experiments. E-Quest is now pursuing demonstrations at laboratories in the scientific mainstream and it is now linked with key scientific laboratories and researchers. The company demonstrated its equipment at Los Alamos National Laboratory and at SRI International during the fall of 1993 and again for sereral weeks in the spring of 1994. It took advantage of these demonstrations to explore new avenues. Not all results with new approaches worked as expected. Some approaches led to experimental dead-ends. Of course, word of every "failure" of new experimental protocols filtered through the scientific network to be debated and discussed. But at each step, the E-Quest team successfully reproduced its "core" results—the excess power and the the under water melting. Steadily it has gained the respect of other researchers. The E-Quest experiment, with it's unusual approach and tantalizing results, often proved astonishing and compelling to people who were initially very skeptical. After all, here was one experiment that had everything come together: excess power in abundance, nuclear "ash" in the form of helium-4 (as well as trace amounts of helium-3 and suspected isotope changes and element transmutations in the metal), and high reproducibility.

The connections with other researchers has been especially important. As a small group, with limited resources, they have tried to develop what what Russ George calls their "virtual laboratory" — a network of researchers around the world who do collaborative work with diverse talents and equipment. The network operates in concert toward gathering a large body of knowledge about the micro-fusion effect. Of course, most conventional scientific work is approached in a similar way; laboratories work cooperatively all the time. The difference in this case is that the E-Quest group *consciously* pursued the strategy. This approach has proven to be especially important for a small group taking on what could become a huge technology.

Because of the great potential of the technology, E-Quest also made the crucial decision not to try to focus initially on developing and selling power-generating commercial devices, but to try to license the technology to manufacturers. The company wants to stick primarily with research, stay small, and concentrate on developing the technology for later licensing to manufacturing companies. "We don't want to try to sell commercial gadgets," says Russ George, "it would grow far too big, too fast for us to handle. We'd rather do discovery research and license our technology to other people."

The future

E-Quest's research work proceeds apace. Just as this issue went to press before ICCF5 (the Fifth International Conference on Cold Fusion), new results from a recent series of experiments were being analyzed. They were discussed at the Sci-Mix poster session of the American Chemical Society Annual Meeting in Anaheim, California (on April 3). Substantial input is also coming from other researchers. New reactor designs—beyond the Mark II—have been constructed and a concerted effort is underway to increase the heat output of the reactor, based on what has been learned from recent experiments.

While the scientific results and data are exciting, the next goal is also within view: developing prototype devices for experimental research in heat production, which will allow scientists to get to the bottom of the evidently complex physical phenomena in "cold fusion." By contrast, developing commercial devices is likely to be much more difficult; the engineering obstacles are formidable. For example, designing the device to run for long periods without destroying the metal targets has not been easy. Consider what will happen when, as seems inevitable, large industrial firms become convinced that the process is real. It's a good bet that their hundreds of talented engineers could burn right through the obstacles —much as four years ago beneath a grove of redwood trees, ultrasonic "micro-fusion" burned through its first palladium target in Roger Stringham's basement lab.

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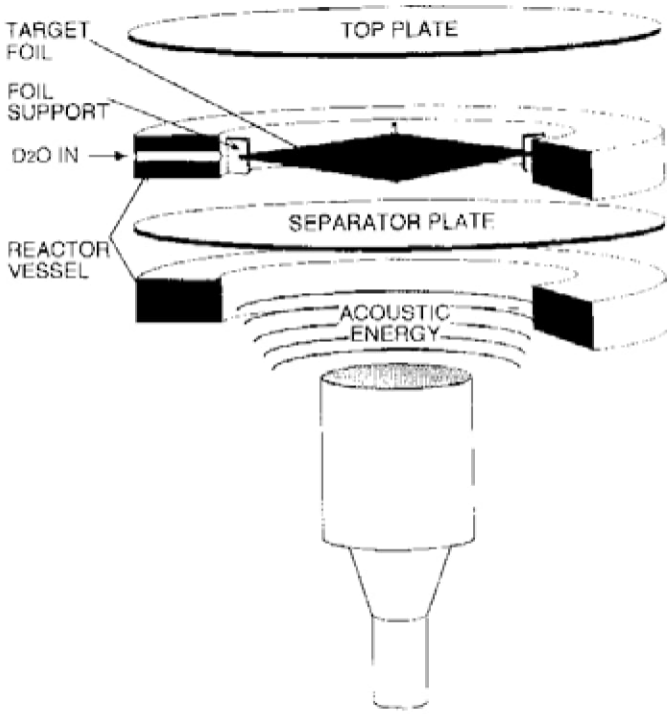
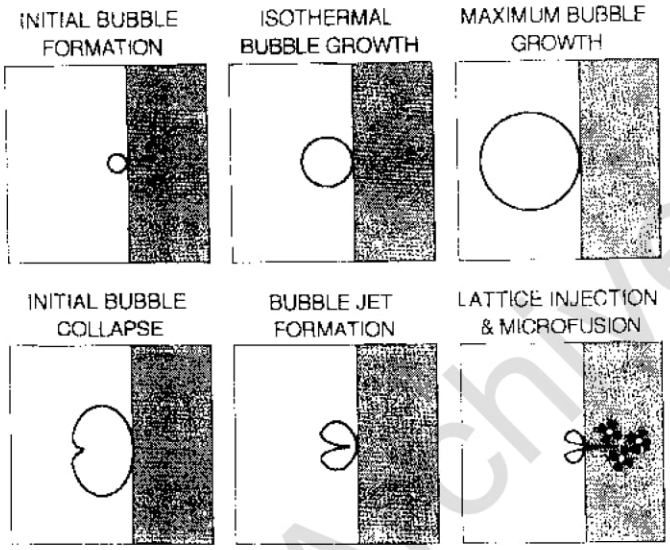
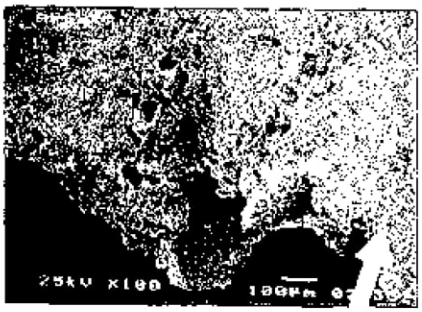


Schematic of E-Quest Micro-fusion Reactor

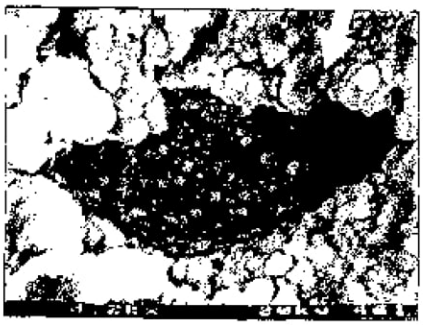


Schematic of cavitation bubble formation and collapse

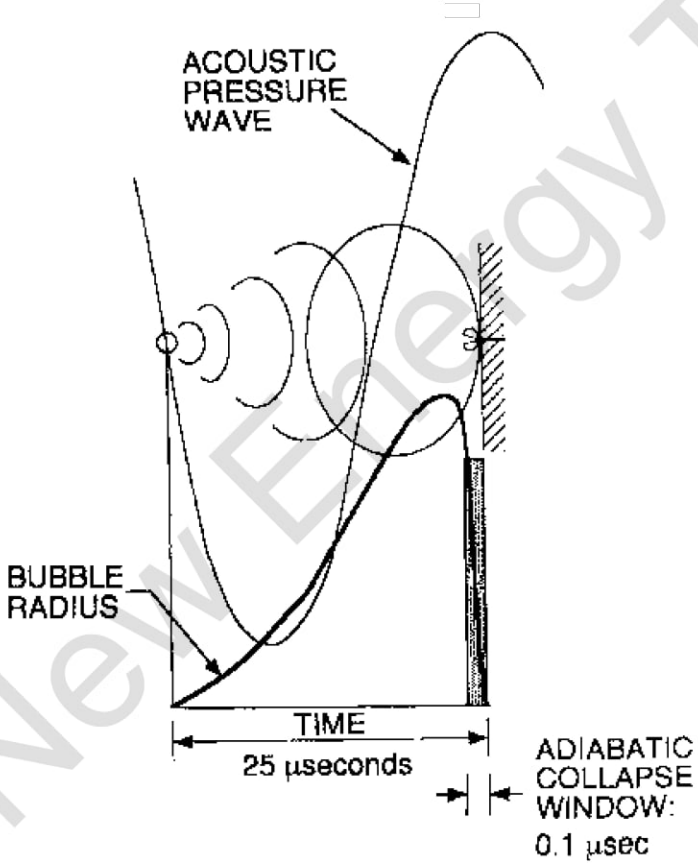
Photograph of a palladium foil with a large central "melted" hole. Foil dimensions 50mm x 50mm x 0.1mm. Mass about 3 grams. Approximately 1/4 of the foil has completely melted and/or vaproized. The remaining foil shows clear evidence of melting and recrystalization.



A scanning electron micrograph (100X) of another Pd foil that has been involved in a "melt." This image is near the edge of the central hole. Features visible include numerous event sites where hot material has been ejected from the surface via eruptions of molten and gaseous Pd.



This high magnification (3200X) SEM image shows an event site from the above sample where small micron-diameter palladium spheres are visible, fused to the bottom of the event crater. This is characteristic of an event in which molten Pd was ejected from the lattice leaving behind sputtered Pd beads.



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From a Sea of Water to a Sea of Energy?

An Adventure in Hands-On Experimental Science

by Michael T. Huffman, marine engineer

The Vacuum and the Polar Bear

From the very beginnings of Eastern philosophy, through the Persian mathematicians' definition of zero, to modern science's efforts at understanding the nature of the universe, we repeatedly find references made to the concept of Nothingness, the Void, the place where nothing exists. The basic promise of the Eastern mystic is that if you silence all of the thoughts, you will find enlightenment. A common notion of many modern scientists is that if you pull everything out of a volume of space, you can create a physical environment where a lot of pure science can be done. Scientists call this empty space the Vacuum.

Given the proper tools and materials, it would seem to be a relatively simple matter for a scientist to create this environment. If the concept can be proven to exist mathematically, one should be able to create a way to physically demonstrate that proof. The actual creation of a "perfectly empty space," however, has never been accomplished. Every time someone builds a physical means to achieve a vacuum, someone else finds something else occupying what was first thought to be empty space.

It seems, indeed, that Nature simply "will not allow for the existence of a perfect vacuum," just as Nature will not allow the existence of absolute zero degrees Kelvin. The concept of a Void is just that—a concept. For most people it is simply a matter of intellectual convenience to accept the idea of a vacuum. For a scientist, it should be considered as a means to an end. The quest, in and of itself, for creating an empty space has been, and should continue to be, one of the most fruitful methods of discovery in the search for understanding the most fundamental relationships between matter, energy, and time.

I was first introduced to the practical applications of vacuum technology aboard the M/V Polar Bear, a self-powered, self-contained, self-sustaining, fish processing boat. I learned the principles of refrigeration, hydraulics, pneumatics, hydrodynamics, electronics, internal combustion engines, materials science, boat design, and seamanship on that boat. Every year for five years, I would kick that fifty year-old bucket of bolts from Seattle, Washington up to the remote regions of Bristol Bay, Alaska. I would freeze two or three million pounds of fish in six or seven months, turn around, and kick what was left of the boat back down to Seattle. Every year I had literally a crash course in physics, yet there was nothing academic about it!.

All of my actions and decisions involved survival and big money. The Polar Bear had a desalinating device made by the Maxim Company to convert sea water into drinking water. The Maxim™ used a combination of high pressure pumps, and eductors to pull a partially filled tank of seawater into a vacuum. Two copper coil heat exchangers were positioned inside the tank, one on top of the other, separated by a V-shaped trough. The lower heat exchanger was piped into the radiator of an auxiliary engine to provide enough heat to boil the water in the tank. The upper heat exchanger was piped into the cold incoming seawater line, which allowed the pure water vapor to condense and fall into the trough. A small, two-stage pump would draw the distilled water out of the trough. The remaining brine solution in the lower portion of the tank was drawn off by an eductor positioned near the bottom of the tank, and it flowed overboard. After being preheated by the upper heat exchanger, new seawater was introduced to the tank via a parabolic-shaped jet valve which would spray the seawater in tiny droplets just over the lower heat exchanger and below the V-shaped trough. By maintaining a negative tank pressure of 27 inches mercury, it was possible to boil the water at as low as 140°F. The breaking up of the seawater into tiny droplets greatly facilitated the evaporation.

The Maxim™ was a very well-designed desalinator, but it suffered from two problems. First, the model that we had simply wasn't big enough. From the manufacturer's specification, it would only produce 385 gallons of drinking water per day. The Polar Bear normally sailed with 24 crew members. Now, 385 gallons of water a day for drinking, bathing, laundry, cooking, and cleaning for 24 people was little enough. I also had to supply about 10 small fishing boats with fresh water. Each one of those boats normally had four crew members. There was just not enough water to go around. Showers and laundry were severely rationed even when everything worked right. When they didn't, suffice it to say that fixing the problem was a high priority! The second problem was that the Maxim(TM) was old, and many of the parts could not be rebuilt or replaced. This meant that the amount of time spent monitoring it's performance, and doing minor repairs amounted nearly to a full time job. Every time I turned my back on the machine, it would stop working. This happened so regularly, that I almost began to believe in the existence of poltergeists!

The Schaefer Machine

It was at about this time that my father told me about a machine that had been invented by a man named Karl Schaefer that could efficiently distill water. I was so motivated to replace the Maxim™ that I flew to Chicago to see this new machine. When I got to there, my father called Sheldon Hughes, the engineer who was working on the device. We arranged to meet and Sheldon would drive us to the lab where the device was being tested.

There was another engineer, David Drummond from Australia, who was visiting my parents. He went along as well. David had seen the device in operation before, and was very excited about it. David is also a marine engineer, but with much more experience than I have. He said that he had worked on thousands of different technologies, but had never seen anything as powerful or interesting as this Schaefer machine. As we drove to the lab, my excitement and curiosity was building. When we arrived, we were allowed to examine the device close up. It was a fairly large machine, the case being about two-feet in diameter and one-foot thick. It was driven by a 50 HP, three-phase electric motor. The outlet pipe was plumbed into a large condenser unit that was approximately two-feet in diameter and twelve feet long.

I remember commenting that the device must really boil a lot of water to need that large a condenser unit. Sheldon told me that it was capable of converting about 5 gallons of water per minute into steam. That was exactly the kind of performance I was looking for, and I wanted that machine for my boat that minute. The drive train consisted of pulleys and a V-belt. Everything looked very nicely constructed, wired, and plumbed— except for one small detail. While examining the device, I noticed that the pulley on the rotor shaft was held in place with a taper-lock bearing. This is normally a good method, but this one didn't have a key in

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the keyway. It relied on pressure alone to keep it in place. I pointed this out to the lab engineers, but they said that they ran it that way all the time, and that it was fine. They then asked David, Sheldon, and myself to remain in an enclosed observation balcony for the test run. I wanted to see the thing run up close, but I was glad that I didn't. When they turned it on the motor came up to speed, and the pulley flew off. It ricochetted around the room at a very high speed, narrowly missing one of the lab technicians. It finally flew out of an open door, and we ran outside to find it spinning on it's side in the middle of the street! David turned to me and said in his wonderful Australian accent, "Well, it looks like you were right about that taperlock bearing, mate!" The lab engineers immediately announced that the test was over, and ushered us to the front door. Needless to say, I was more than a little disappointed. I traveled all that way, and never did get to see it run.

On the drive back to my parents' house, David and Sheldon described various abnormal aspects of the machine's performance. They talked about measurements which were said to show that the machine produced more heat energy than the equivalent in mechanical energy needed to run the machine! They described the steam as being almost dry, and having a bluish tint to it. They said it could reach temperatures as high as 700°F. All of this talk frustrated me enormously. I knew that the machine was still in the prototype stage, and that it could easily be years before I might see a production model. I also could tell by the appearance of the machine that when it finally did get mass-produced, it would be expensive, which meant that my chances of ever being able to tear one apart were that much smaller. When we arrived at my parents' house, I badgered Sheldon for more details about the construction of the machine. He drew me a picture of the rotor on a napkin and described in detail all of the measurements and how the thing worked. I understood the concepts immediately. I was shocked at the elegance and simplicity of the design, and amazed that no one had tried this before. I remember thinking, "Even I could build one of those!" The next day David and I drove down to the Purdue Engineering Library, and got copies of the Schaefer patent. After reading the patent, and looking at the drawings, it was obvious that while the concepts of operation were the same, the rotor design that Schaefer patented, and the one that Sheldon drew on the napkin were very much different.

I flew back to Seattle with a mixture of excitement, frustration, and puzzlement. I wanted to build a machine and try it on the Polar Bear. I showed the Schaefer patent to Rob Royer, the Port Engineer for North Coast Seafoods, and responsible for all of the boats in the fleet, including the Polar Bear. With over twenty years of experience on Alaskan waters, and having been consulted on the design and repair of literally thousands of boats, he is considered to be one of the best engineers working today. A man of no small ego, he is the first to tout his experience and qualifications. After getting to know him, and seeing the quality of his work, I came to have great respect for his judgement. For all his arrogance and scathing attacks on the incredible ignorance and stupidity of mere mortals such as myself, I found him to be right about 98% percent of the time. I thought that it would be easier to pull the back molars out of a sea lion's mouth with my bare hands than it would be to get Royer to admit that he was wrong about anything. He could build, redesign, or fix *anything*, and no matter what curve ball Fate and Nature threw at us.

Rob took a look at the patent, and listened to everything that I told him about the machine. He said that it might work, but until it was built and tested, he wasn't going to give up the Maxim(TM). He also said that it was physically too big for the Polar Bear engine room, that the 50HP motor would be too much of a drain on the already overtaxed generators, and that it would take a lot of time to build that needed to be spent on other things. I had to agree on all accounts. He wanted to try replacing the worn out two-stage pump on the Maxim™ with a diaphragm-type pump. We weren't sure how well it would perform with a negative suction head pressure, but we should at least give that a try first. If it worked, it would be a much faster and cheaper solution. He was right again. After about a day of installation, and adjustment, the diaphragm pump worked. It not only worked, it allowed for a deeper vacuum to be created in the tank than ever before. We now had a 29-inches Hg vacuum, and could distill over 400 gallons of water per day. While this was still not enough, it was more than the manufacturer's specifications. With the added reliability, it was an enormous improvement. Of course, Rob crowed about it for at least a week, and I filed the Schaefer patent in my file cabinet for another time.

Being Observant

I didn't think about the machine very much until the 1993 season, when a number events made me attach a greater importance to the concepts of cavitation, the water hammer effect, and the creation of a vacuum. We had a fuel barge called the Boxer that we filled up in Seattle and towed up to Alaska. As the name implies, it was a box-shaped barge that held approximately 135,000 gallons of diesel fuel in two tanks. In the previous season, the Boxer had snapped it's anchor cable in a storm and had been thrown up onto a beach. It took about a month to dig a trench around it during the low tides, and to lighten it up enough for a tug boat to pull it off the beach. When the Boxer finally made it back into the water, we noticed the the hull had developed a small leak on one of its seams. It wasn't bad enough to fix immediately, but over the following six months, it had taken on enough water in the forward fuel tank that it was obviously going to become a problem if not taken care of. We towed the Boxer back to Seattle, pumped the fuel/water mixture into the aft tank, and put the Boxer in a shipyard, where the seam was welded. We then spent a lot of time constructing a larger anchor winch, and installing a larger anchor with thicker cable to prevent it from being blown onto the beach again. Rob hired someone to remove the water from the fuel in the aft tank. We filled the Boxer back up with diesel fuel, and towed it back up to Alaska, thinking everything was OK. Well, everything was not OK, to say the least. The guy that Rob hired didn't get all of the water out of the aft tank, and by the time we discovered it, the nightmare had already begun.

The first boat to experience a problem was the M/V Teal. It is a fish-buying boat that is powered by two GMAC 6-71 diesel engines. After taking fuel from the aft tank of the Boxer, the skipper of the Teal said he was cruising along when the boat suddenly swerved hard to port for a few seconds, and then, just as suddenly, swerved hard to starboard. He looked at the engine gauges, and noticed that the starboard engine had lost all oil pressure, and that the exhaust stack temperature was a whopping 1,200°F! He said that he had just checked the gauges about 5 minutes earlier, and they were normal. He shut down the boat and he and the engineer went down to the engine room to see what had gone wrong. They found the starboard engine had blown a hole the size of a dinner plate through both walls of the block. The piston, liner, and most of the rod were simply gone. You could stick your whole head through one side of the engine and out the other. I was impressed. We checked the fuel tank on the Teal, and found water. We figured that the water blew the tip of the fuel injector on, and the engine just ran away until it finally blew up. We had to fly up a whole new block, head,

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pistons, rods and liners. We managed to salvage the crank shaft, and we rebuilt the engine ourselves.

One by one, all of the boats in the fleet began experiencing problems. Blown head gaskets cracked heads, burned valves, you name it, we had it. In every case, we found water in the fuel lines. All of the engineers were working around-the-clock, just to keep their boats running. I think I changed about 25 head gaskets, 6 heads, about 200 fuel filters, 180 injectors, a number of valves, and completely rebuilt 4 engines that year—just because of water in the fuel. On several occasions, we worked 70 hours without sleep to get an engine back on line. It was truly a nightmare.

On one occasion, I had to change the oil and filters on the main generator engine. I had been working so hard on other things that the oil and filter change was long overdue. I knew that if I waited any longer, we would be without electricity, which is not a good thing to have happen even for a few moments. We had a small back-up generator, which I fired up, and let come up to temperature. I went over the the main electrical panel, and threw the load from the main generator to the back-up generator. When I threw the switch, the main generator took off like a monster. The RPMs climbed higher and higher, and I knew instantly that the engine was running away. I ran over and turned the governor on, but the RPMs still kept climbing. I shut on the valve to the fuel line, but it still kept running faster. I jumped up onto the mount rail, and clamped both my arms over the air intake ports in an effort to smother the engine. The engine bucked so hard that I thought it was going to break the mount welds. It still kept running, and I knew that my legs were about six inches away from the exact spot on the exact same model engine that had blown up on the Teal. I saw a $C0_2$ fire extinguisher out of the corner of my eye and decided it was a much better idea to smother the engine with $C0_2$ than with my body. I jumped off of the mount rail, grabbed the extinguisher, and dumped a load of $C0_2$ into the air inlet ports. The engine died instantly.

By this time Rob had heard the engine, and had come running into the engine room. I sat down on the deck, and shakily smoked about ten cigarettes, while I watched the smiling Rob pull the injectors out of the engine. Sure enough, the injector tip on the number one cylinder had blown off, and was dumping an unlimited amount of fuel and water into the cylinder. I had nearly blown myself up a number of times before, but the combination of the heat and noise made this time especially frightening. This was only background, however, to another frightening experience yet to come.

It was another incident that made me think more about the Schaefer machine. I heard a loud popping noise coming from one of the engines driving a refrigeration compressor. It repeated very fast for a couple of seconds, and then it quit It sounded like a machine gun. All of the gauges were within the proper boundaries, and I was busy with something else, so I walked away from it. It happened again a few minutes later, so I decided I'd better check it out. I checked the oil, and the viscosity seemed a bit off. I don't know why, but I decided to taste it. It tasted like antifreeze, and I figured that I had blown a head gasket. I shut the engine down, and pulled the head, and sure enough, there was water all over the head, leaking into the oil. I also noticed something that I had never seen before. The machined surface of the head had about a million little pits in it. It had the texture of an orange peel. The pits were perfectly round, and shiny clean. I knew that it wasn't rust pitting. I called Rob down to have a look at it. He said that it was from cavitation. I had seen cavitation-pitting on aluminum propellers before, but this looked much more uniform. I also knew that water ceased to be water at around 900°F, and that high carbon steel melted at around 2,500°F. I asked Rob why these two facts didn't seem to add up. He looked at it again, shook his head and said, "I don't know. Just replace that head, and ship it to Seattle. If we're lucky they can mill it down, and we won't have to buy another one." I about dropped my teeth—there was something that Rob didn't know! I decided that if water could cause all that heat, damage, and destruction, then, in trying to harness that power maybe Schaefer had been on to something important.

That was the last year I sailed on the Polar Bear. I decided that I had learned about all I could learn on that boat, and that a more modern boat would probably be a lot less work. I was ready for something different. I called my dad and told him about my decision. He was relieved. He had seen the Polar Bear and knew how dangerous the work was. I asked him how the Schaefer machine was developing, and he said that the company had run out of money. He said that they had gotten some good offers for the technology from some large corporations, but that the stockholders wanted a lot more money for it. As a result, it was going nowhere fast. He said that I should build one and see if I could find a manufacturer for it. I really wasn't in the mood for anything except a long vacation, so I told him that I would think about it.

Getting Hooked

After my vacation, I started thinking about what I wanted to do. It was January of 1994, and the crab and cod fishing season were just getting under way in Alaska. I still had a lot of money in the bank, and wasn't too thrilled about going up north during the winter months. I thought I would just hang out in Seattle and see what offers came my way for the summer months. My dad had called, and he again urged me to build one of those machines. Since I hadn't seen the thing actually work, and since I knew that I would need access to a lot of equipment that I didn't personally own, I wasn't too keen on the idea. My dad sent me several reports in the mail from various universities that had tested the Schaefer device, and had concluded that the device appeared to be somewhere in the neighborhood of and "impossible" 150% efficient. While this intellectually was an interesting claim, I still wasn't convinced that it would be worth my time and money to investigate it.

I thought that since the company that was developing the device already had prototypes and university verification, that it would be able to find the money to manufacture the machine. I was content to just wait until the production models came out. It was enough for me just to know about it, and follow the development from the sidelines. After all, I knew about a lot of emerging technologies, why should I have to build and test them just because my father happened to be involved with one?

About a month went by and my dad called again. He said that the company had folded-up completely. He was now practically demanding that I build a machine. I outlined all of the reasons that I could think of for me not doing it, but to no avail. He was so adamant about the importance of it that finally, out of respect, I caved in and told him I would give it a try. I secretly resolved, however, that I would spend as little time and money on it as possible. I had plenty of other things that I thought were more important to do. I began reading everything I could get my hands on about vacuum, cavitation, and the water hammer effect. My sources were pretty general, but I thought that I knew enough to build a simple device. In my time in Alaska I had rebuilt about 30 different kinds of pumps, and this wasn't much different than a simple rotary pump. I decided early on that if I could build a machine that could heat water to a temperature of 140°F at a rate of three gallons per minute, that it would have enough applications in the home construction or recreational vehicle industries

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to be attractive to a manufacturer. It also had to be cheap and easy to build, operate, and maintain. In my mind, I slowly began to assemble the device.

After reading everything that I could find, I decided that the machine that Sheldon had described to me wasn't taking full advantage of the physics involved, as I understood them. I had many ideas about the rotor design that I thought might be serious improvements. In fact, I thought that if the improvements were significant enough, I might apply for my own patent. I decided that I would build a small one out of clear plastic, so that I could see what was going on inside the machine, and get a feel for how the thing worked. I bought a 1/3HP Dayton™ motor, a shaft seal from Grainger, and one gallon of casting resin from a local art supplies dealer. Along with some nuts, bolts, and some gasket material from a hardware store, I had enough to make a prototype. The total cost of materials was around $150 (US). I used a coffee can as a mold for the rotor disk. I then used a cake pan as a mold for the housing pieces. Within a week, I had a blank rotor and all the housing pieces cast, cured, and ready to assemble.

The first thing I wanted to know was how the blank rotor performed without any holes in it. I assembled the device in this manner, and turned it on. I was using the kitchen sink faucet as a water supply, a dial thermometer to test the temperature, a Pyrex™ measuring cup to measure the heated water output, and a stopwatch to time the flowrate. I wasn't concerned about the efficiency of performance or the sophistication of the test, I just wanted to see if it would even heat the water. After running it for about a half an hour, I managed to raise the temperature of a tiny trickle of water about 7°F. I thought that if the modified rotor performed even close to this poorly, I had just thrown away $150 bucks! I took the thing apart, and proceeded to drill holes in the circumferential edge of the rotor disk. The disk itself was roughly 3.875 inches in diameter, and just under 1 inch thick. Sheldon had told me that he was drilling 1/8-inch diameter holes into his rotor to a depth of about 1-inch. I decided to do the same. I drilled about 32 holes into the disk, reassembled the device, and turned it back on. This time I managed to raise the temperature of the water about 22°F. Still not too impressive, but I knew that the holes made a difference. I took the thing apart again, drilled more holes, reassembled it, turned it on, and got more heat. More holes = More heat. Sounded reasonable to me.

I proceeded to put as many holes as I could fit into the edge until I ran out of room. I then switched to a smaller diameter drill bit, and found enough room for more holes. Each time I added holes, the temperature of the output water would increase. I finally reached the point where I didn't have room for any more holes. The Delta-T —the temperature between inlet water and outlet water— was still only about 50°F, which was not good enough for me. I went back to my physics texts to look for a way to make it better. I reread a section of on the mechanics of materials that had to do with centrifugal force. It appeared to me that by drilling the holes deeper, I could not only lighten up the rotor, but I would be adding to the overall mass of the water being pulled by the centrifugal force. Since what I was trying to do was create "enough of a vacuum to pull the water molecules apart," it made sense to drill the holes as deep as they could go. I tried this on a number of holes, and got more heat. I then drilled all of the holes as far as they would go. The final Delta-T was increased to 70°F, ten times as much as with the blank rotor!

While I knew that the flow rate and temperature weren't high enough for any practical applications, I knew that by making the rotor larger in diameter, or using a higher speed motor.to get more centrifugal force, I could get the throughput up to whatever I desired. I decided that the machine was definitely worth pursuing, and that I should probably get some kind of intellectual property protection on it before I did anything else. I made an appointment with a patent attorney.

Patents and Progress

I gave the attorney my prototype, and the Schaefer patent. I described how I thought the thing worked, and described the work done by Sheldon Hughes. I wanted to know if what I had built was different enough to warrant patent protection. My attorney recommended a thorough patent search of all the patents that worked on this principle. He said that he employed an excellent search firm in Washington, D.C., and that they normally took from two to three weeks to find any and all of the related patents. He said that he could give me his opinion after that. I agreed to work with him and went home to wait. During that time, I did a lot more reading and thinking about how to make the machine better. I knew that to create a deeper vacuum in the holes, I needed to increase the centrifugal force. I knew that with the present design, I was limited to making the holes a certain diameter and length. This left me with the options of increasing the diameter of the rotor, or increasing the speed of rotation to create more centrifugal force. I was staring at the formula for centrifugal force, $F=mV^2/r$, when it dawned on me that I could increase the mass of the water in the holes simply by curving the holes. This meant that the diameter of the rotor itself did not have to be increased, which would allow it to be as small and light as it presently was. I also thought that perhaps, if I curved the holes in the direction opposite the direction of rotation, that I might get some propulsion from the expanding steam pushing its way out of the hole— like an inverse turbine. I spent the rest of the waiting period dreaming up rotor designs based on curved tubes, and incorporating aerodynamic structures that would take advantage of the water movement in the tubes. I felt that the number of possibilities for rotor designs were endless, and that I could easily spend a number of years trying them out.

After three weeks had gone by, I called my attorney to see what the patent search had turned up. He said that he still hadn't received anything from the search firm, and that he would call them the next day to see how they were coming along. I was a subscriber to the GEnie online service and I had stumbled across the World Wide Patent database. I was getting a little impatient, so I decided to try searching myself. I soon found that while there were a lot of patents for rotary water heaters, all of them used friction or shear forces to create heat. I finally ran a search on the Schaefer patent and I discovered that it had been cited in another patent application. I called the people that maintained the database, and asked them if it was possible to find the patent that had cited Schaefer's work. They said that they maintained a separate cross reference database that was not available on GEnie, and that they could send me the full text in a few days. I got the text a few days later for the patent that had been filed by James Griggs.

From the text, it sounded like the machine that I built could be covered by Griggs' patent, but I wasn't sure. The events that followed immediately were such a coincidence that I must relate them. My brother had called that day to tell me that he was changing planes in Seattle the next afternoon, and asked if I wanted to meet him at the airport for a cup of coffee. I agreed. I went with a friend early the next morning to the University of Washington Engineering Library. They had copies of all the patents, complete with the drawings on microfilm. After locating the correct reel, I found the Griggs patent and drawings. One look, and I knew that his machine was very similar to the one I had just built. I made a copy of the patent, threw it into my briefcase, and headed for the airport to meet my brother. My friend wanted to stop

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at a newsstand to look for a computer magazine. I was in a hurry, but said OK. I was browsing through the science section when I came across the *"Cold Fusion"* Magazine that Gene Mallove was then editing at Wayne Green, Inc. I had never seen it before, and was interested. What caught my eye, however, was the article by Jed Rothwell and Gene Mallove on James Griggs and his Hydrosonic Pump™! I grabbed the magazine, and headed for the cashier. I grabbed my friend as well, and told him I was late for my brother's plane. We raced down to the airport, and ran towards the gate at which he was supposed to arrive. There was a guy in the airport sitting at a card table covered with magazines and books He saw me and said "Hey! You look like an engineer, come over here and read this!' He was selling *21 st Century Magazine*, and on the cover was Griggs' name again! I bought that magazine too, and ran to meet my brother. I got home later that afternoon to find a message on my answering machine from my attorney. I called him, and he said that he had just received the results of the patent search. He said that there were a lot of patents that weren't like my machine, but there was one by James Griggs that looked like it might be similar. He wanted to see me the next day. In one day, references to the Griggs patent popped up four times! He seemed to be a pretty popular guy.

Building the Dream Machine

I met with my attorney the next day, and we went over the Griggs patent, line-by-line. We both concluded that while my machine was different in a lot of ways, it was too similar to apply for another patent. I told him it was no problem however, because I had a million ideas that I thought were better, and that I just needed to build and test another one. I immediately went about doing just that.

I had been reading a little bit on different prototyping methods. One method that seemed to fit my needs was called the 3-D layering technique. I decided that if I used clear acrylic plates, I could solvent-weld them together in stacks to make my machine. I could use a small hand grinder to carve out the curved tubes in the rotor, and shape the inside of the housing pieces. While I knew that this would be a lot more work than just drilling holes into the rotor, I also knew that once the principle had been demonstrated, a manufacturer could easily and cheaply reproduce the machine with mold-injected plastics. In fact it would be easier to make than a plastic cigarette lighter, and not much more expensive in terms of materials. I called a plastics distributor, and got a quote for 135 clear acrylic plates, measuring 6"x 6" x 1/16", and a couple of cans of solvent welding glue. They said it would cost about $70, and they could have them cut in three days. The 135 plates were enough for me to build two machines, with some to spare. I then found a Dremel™ Rototool on sale at a local hardware store for $59. I used the Dayton™ motor, the shaft seal, and the pipe fittings from my earlier prototype. This and a few more nuts and bolts, and I was in business! All for under $150.

I had made templates with a shareware 2-D CAD program, and printed them out on my dot-matrix printer. I just laid the acrylic sheets over the templates, and traced over them with the Dremel™. When the plate was carved, I glued it together with the rest of the carved plates in a stack until the machine was finished. Each rotor plate took approximately 14 hours of work to carve. I designed the rotor to be 4.3 inches in diameter, and 15/16-inch thick. It took me two weeks to build the rotor alone. The housing plates went a little faster, but not much. All in all, I spent six weeks gathering the materials, carving the plates, and assembling the device. I had thought that I could cut the rotor plates out of the square plate, and then use the square plate with the hole in the center for the housing piece. If I was careful, I would only have a gap of 1/16-inch between the circumferential edge of the rotor and the inside of the housing. Since I was shaping everything by hand however, the rotor ended up being only 4.2 inch diameter, and the gap between the housing and the rotor was more like 1/8-inch. When I assembled the machine, and tested it, I could only raise the temperature of the water 90°F. While this was better than the previous model, I felt that I could improve the performance a great deal by making a new housing piece with a tighter gap between it and the rotor. Sheldon had insisted repeatedly that no matter what size rotor he had made, a gap of 1/10-inch resulted in the best performance. He made it sound like it was some kind of universal constant, but since all of the rotors that he made were over 12 inches diameter, I felt that my gap should be proportionally smaller. I set the housing piece aside, and started carving a new one that would be as tight as I could make it. I figured that if I started around 1/32-inch, I could test it, and trim it out to a larger gap until the performance started to fall off.

I finished the new housing piece, and reassembled the machine. I hooked it up to the sink, and turned it on. I had a thermometer stuck in the outlet port, and I watched the temperature go up to about 200°F in about three seconds, with steam coming out of the port shortly thereafter. That was more like it, I thought! I heard a slight tapping sound coming from the front plate of the housing, and I noticed a small glob of plastic forming on the inside of the plate, so I jerked the plug, took it apart, and found that the rotor was loose on the shaft, and was hitting the inside of the front plate.

At this point I should explain that I'm a very careful mechanic. I had put this thing and the previous model together many times, and I had always gotten the rotor very snug on the shaft. I also always checked for any rubbing between the rotor and the housing before turning it on. I was very sure that this time was no exception. Since the shaft was threaded in a clockwise fashion, and the direction of the rotation was counterclockwise, the only thing the rotor could do was get tighter on the shaft as it ran. That is, I thought, unless the rotor was *outrunning the motor*. I had hoped for a little propulsion with this design, but having the rotor unscrew itself, I thought, was simply impossible. I concluded that I must have done something wrong, and proceeded to clean the glob of plastic out of the machine. The damage was minimal, so I put the thing back together. This time I tightened the rotor up securely, and made SURE to check for rubbing between rotor and casing. I turned it on, and the very same thing happened again! First the tapping noise, the rapid rise in temperature, then the formation of plastic on the inside of the front housing plate. I turned it off, took the thing apart again, and found the rotor had unscrewed itself. I didn't know what to do at this point. Up till now, I had been tightening the rotor on to the shaft with my hands alone. I decided to use something a little more drastic, and get some leverage on it. I got some larger tools, and tightened it this time until I could feel the threads mash. There is a thin line between mashing the threads, and stripping them, but I could tell that this was as tight as I could get it.

This time when I turned it on, the threads held, and the thing took off like a rocket. It came up to speed, seemed to hang for a split second, and then powered through it. The RPMs climbed higher and higher–constantly accelerating. I was frozen to the spot with fear. Judging by the sound of it, the rotor was doing at least 8000 RPM, and going increasingly faster. I watched as great big globs of plastic formed all over the inside of the machine. After about 9 seconds, I regained my senses and pulled the plug. I almost cried.

The rotor had melted so badly that I thought it was a total loss. The top part of the rotor was pushed up against the rear housing plate, and held in place by a big glob of plastic from the front. The bottom half of the rotor was pushed up against the

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front housing plate, and was held there by a big glob of plastic behind it. The threaded portion had backed off of the shaft threads again, and melted into place. It was fused to the shaft so that I couldn't get it off. Big globs of melted plastic were all over the inside of the housing. All of my work was ruined! I remember thinking at the time that the thing sounded just like that runaway engine. Then it dawned on me—that runaway ENGINE! The machine was some kind of engine. I wasn't so "bummed out" about melting it now. In fact, I was so stunned by the idea, that I just stumbled around my apartment for about three days thinking about the ramifications of what this could mean for the world. Finally, I called my attorney and told him that the new prototype worked, and made an appointment to see him again. I didn't tell him how well it worked.

I ran to the patent attorney the next day, and we decided to file a Statutory Invention Registration. This would be cheap, and it would protect me during the time it would take to build a new machine and write the formal patent application. The SIR took about a week to write. I included the set of mechanical drawings that I had made, and my attorney filed it in July 1994. We then proceeded to begin writing the actual patent application. My attorney asked me a lot of questions about the design and operation of the rotor. He took notes as I told him that one of the key features was it's ability to achieve a propulsion effect when the water changed phase and became steam. He asked how I had measured this effect, and could I physically prove that it was happening? I then broke down and told him the complete story about what had happened to the rotor. I told him that the device appeared to display qualities that were like an engine. As I was telling him about it, I became more and more excited, and the words all came out in a big rush. My delivery was completely lacking in the cool detachment of a professional. In short, I found myself ranting. The attorney waited until I was finished, sat back in his chair, and put his pen down. He had a concerned look on his face like he was thinking that maybe he should call the building security people, or that he was wishing that he had brought the dart gun with the elephant tranquilizers to work. Finally he asked me "Do you mean like a perpetual motion machine?" I said no. I had read about claims of perpetual motion machines. I knew that most of them relied on friction-free, drag free environments, and that all they did was spin around for what seemed to be an unusually long time until they finally came to rest. I knew that the combination of the motor rotor, two sets of ball bearings, the device rotor, and the shaft seal of my device, all added up to quite a large amount of mass and friction. I told him my machine behaved more like a fuel-driven engine, and that it was powerful enough to power through the stator coil magnetic fields, and outrun the electric motor driving the rotor. I estimated that it would take 2 to 3 HP to do what it had done.

My attorney said that unless I could describe exactly what the mechanism was that was fueling the device, that the patent office wouldn't accept an application for an engine driven simply by a low pressure water source. He said that they would view it as another perpetual motion machine, and file it in the circular file—trash it. I asked him if they would accept it as a mechanical water heater, and he said that if Schaefer and Griggs could do it, then he saw no reason why I couldn't do it as well. So we decided to patent it simply as an efficient water heater.

I went back home that afternoon to try and disassemble my melted machine. I would need the motor and whatever else I could salvage for the next one, so this one had to come apart. I struggled to get the rotor off of the shaft for several hours. It was really stuck to the threads. Finally, using some pretty big tools, I managed to unscrew it. When I got it off, I noticed that it had blown two large holes, about the size of a dime, through the rear wall of the rotor. There were several other smaller holes, and a number of bubbles in the plastic that looked like they had been getting ready to blow out when the machine stopped. It was a real mess, but I had an idea that might allow me to patch the thing up, and actually salvage it. I trimmed out the inside of the housing so that the gap wasn't so small. I figured it wouldn't work as well, but at least it would still heat the water. Then I added a number of plates to the housing to accommodate the warped rotor. I reassembled the device, and turned it on. The warped rotor made the whole device vibrate quite a bit. It took a couple of minutes for it to reach the boiling temp, and it wasn't self powering, but it still functioned as a water heater. I carefully regulated the water temperature so that the machine just got hot enough to melt the plastic, then I added more cool water to let it cool back down. I did this several times until the centrifugal force of the rotation brought the momentarily melted disk back into balance. Now the disk was more or less parallel with the inside of the housing plates, and the machine ran very smoothly again. I trimmed up the blown out holes in the rotor, punctured the bubbles, and sanded down the rear surface of the rotor until it was as flat as I could make it. I then carved five more plates, and glued them to the rear of the rotor to patch the holes. The front housing plate was completely warped, so I just made a new one, this time a little thicker. I now had a functioning machine again. While it didn't work nearly as well as before, it was at least something to show people.

The attorney and I worked on the patent application for about three months. It seemed to take forever. In the middle of the process, I had come up with an idea for a friction-free shaft seal. I built it, tested it, and it worked very well. I wanted to include this as part of the design, which meant rewriting quite a bit of the application. Finally, after going around and around with my attorney over the wording of the application, I got tired of looking at it, and told him just to file it as it was. That was in October 1994. By December, the US Patent Office sent me notification that my patent had been allowed. Since this was my first attempt at filing a patent application, I was a very proud and happy man. I thought this must be how first time fathers feel. I wanted to pass out cigars! I wanted to shout my joy from the rooftops. My attorney and I quickly got the formal drawings and the final issuance paperwork together, and sent them in. We are now waiting for the formal announcement by the patent office, which should come any day now.

Connecting

In the month of November, I started looking around for an Internet provider. I had read a great deal about the "Information Superhighway," and thought that it might be a good place to start some kind of discussion about my machine. I felt that by exposing the machine to a large number of people, perhaps someone with a better physics background might be able to figure out what was going on inside of it. I was trying out a local Internet provider called Eskimo North, when I ran across a homepage done by Bill Beaty, an electrical engineer. It had information on Griggs and the Hydrosonic Pump on it, as well as a number of other anomalous behavior devices that I had never heard of before. I E-mailed Bill Beaty and described what I had built. He wanted to see it right away. Bill told me about the sci.physics.fusion Usenet newsgroup, and said that it was "where all the action was." He also said that the cold fusion scientists were increasingly interested in what they called the "Griggs Gadget," because many of the same theories overlapped with cold fusion theory. I put a small, one paragraph post on the newsgroup describing my machine as a "table-

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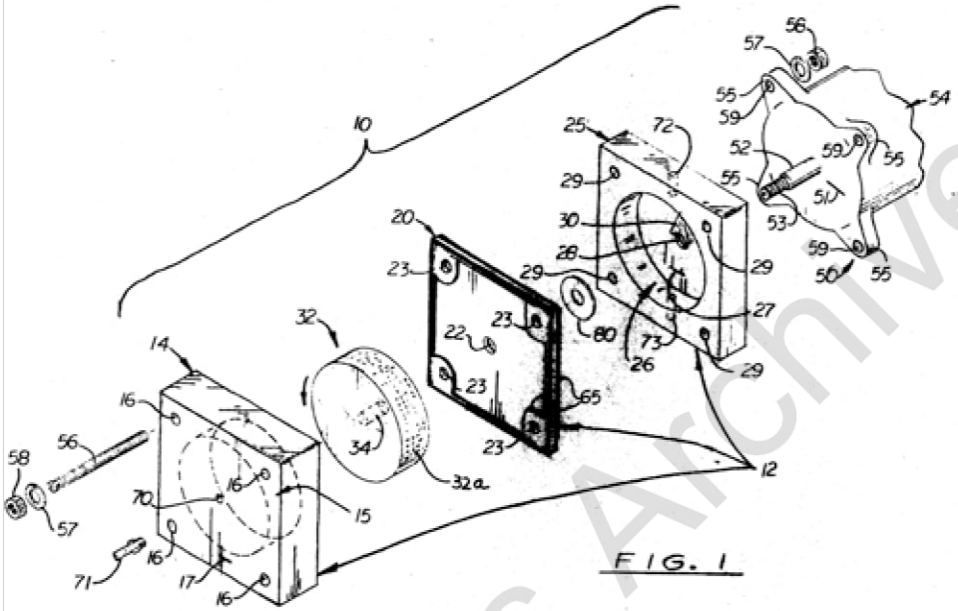
top Griggs-type device." I wasn't prepared for the response. I received over 100 E-mail queries and about 20 phone calls in four days! Up to this point, I had been working completely alone, and I'll tell you, at times, I really *felt* alone. Now I was swamped with people asking very intelligent questions, and advising me as to what I should do next to verify performance or to improve it. I ended up learning a great deal from these people in some of the E-mail discussions that I have had with them. I concluded that getting on the Internet was perhaps my best decision.

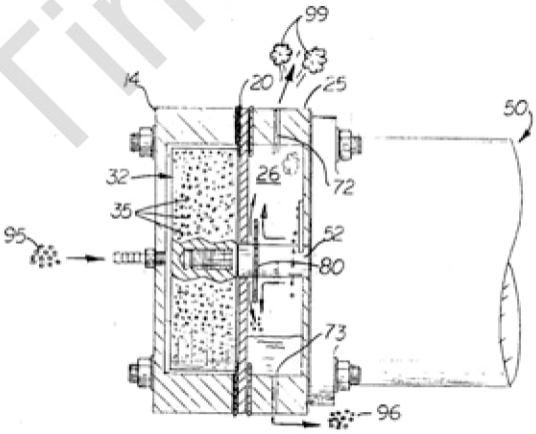
One of the first people who called was Fred Jaeger from ENECO in Salt Lake City. He made an appointment to see the device, bringing Dr. Yan Kucherov and Dr. Oleg Finodeyev along to run some tests. After running the tests, their initial conclusion was that the device was well under 100% efficient. They estimated that the electric motor was around 85% efficient. I had called Dayton Motors the day before the test, and they were good enough to fax me copies of the performance curve data for that particular model motor. Dr. Kucherov had been doing all of the calculations, and so I gave him the Dayton performance data. He recalculated, and the results came out quite different. We had done 5 test runs at various flow rates and temperatures. Three of the runs came out around 95% efficient. The other two were over 100%. One of the tests showed 131% efficiency. Not too bad for a handmade, melted down, wobbly, piece of work.

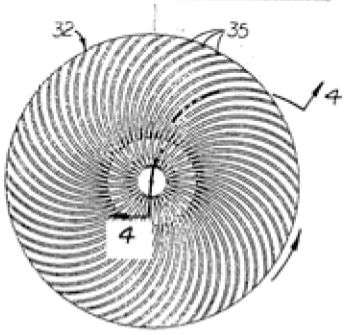
I could tell that Yan and Oleg were initially not too impressed with the device. Admittedly, it looked pretty rough after being melted down. Also the testing conditions were very crude, and there were so many variables that the probability of error in the measurements far exceeded any demonstration of "over-unity". Still, Fred thought the project was worthy to pursue to a scientific conclusion. I managed to convince him that with a little funding, I could have a machine built out of high temperature polycarbonate material by a machinist, which would run circles around my handmade device. We finally reached an agreement to do just that in return for a time-limited, exclusive negotiation right to a percentage of the patent ownership. I couldn't have been happier.

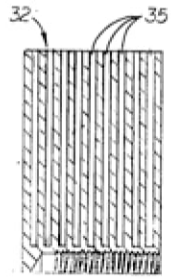
By the time many of you read this, the new machine should be finished and the initial tests run. We had hopes to have it done in time to take to the ICCF5 Conference in Monte Carlo, but the machine won't be ready until just shortly before. I'm hoping that I can have enough test results gathered to fax them to the ICCF5 conference, so stay tuned.



Exploded View of Michael Huffman's Steam Machine - from the patent appplication.







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EarthTech International is a research organization devoted primarily to investigation of alternative energy sources with special emphasis on the extraction of useful energy from the zero-point field. Part of this effort involves the evaluation of devices built by others which are reported to exhibit an apparent over-unity energy balance.

As a result of our own research, we have developed considerable expertise with energy balance measurements on both mechanical and electrical devices. We have a digital oscilloscope which provides accurate electrical power measurements on a wide variety of waveforms.

We have also designed and built dynamometers for mechanical energy measurements, as well as several calorimeter systems ranging in power level from a few milliwatts to tens of kilowatts. Our latest development along these lines is a fully computerized differential calorimeter with total energy integration on both the input and the output. This system is well suited for experiments with time-varying inputs and outputs...such as cold fusion cells.

In the interest of science and the potential benefit to mankind, we hereby offer, free of charge or obligation, to replicate heat or output energy measurements on any cold fusion cell or other device which has been shown to have an apparent excess energy balance. If you will make the device available at our laboratory in Austin, Texas, for a reasonable period of time, we will conduct an extensive series of measurements and prepare a publication-quality scientific report.

At the conclusion of our measurements the device will be returned to you along with the report. We will respond constructively and completely to queries about our measurements from interested parties. For those cases where over-unity performance is confirmed by our measurements, we stand ready to offer support for further development.

Accuracy in Thermocouple Measurement

[Reprinted with permission of **Strawberry Tree Data Acquisition,** Sunnyvale, California
(Phone: 800-736-8810)]

For temperature measurement, thermocouples are the most commonly used sensor. Although they are a rugged and versatile temperature measurement device, due to their low signal level, they are particularly prone to noise and ground loops. To reduce these problems and to help improve accuracy, the engineers at Strawberry Tree have compiled the following list of actions to take when setting up thermocouples:

• Ensure that the negative terminal is shorted to the ground terminal. (Do not do this if the thermocouple is also grounded).
• Make sure that the type of thermocouple chosen in the software is the same as the one in use. Thermocouples can be identified by the colors of their wires.
• Use the isothermal block terminal panels. Without it, errors of up to 5°C can be found in certain environments.
• Make sure that there are no intermittent or solid grounds at the tip of the thermocouple. This can occur in conducting solutions, or in high vibration or dirty environments.
• Do not use very long or thin wire, as this can increase the resistance in the wires and lead to errors, especially at high temperatures.
• Use the low noise mode or at least the highest resolution possible. This will help filter out unwanted noise.
• Make sure that the calibration setup is engaged in the software. This will help prevent errors due to changing ambient temperatures.

If you have tried all of the above and are still receiving inaccurate measurements, there could be a problem with the equipment in your setup.

To check that the software, board, and terminal panel are functioning correctly:

• Connect the positive and negative terminals of the channel in use to each other with a copper wire. The software should read room temperature. If it does, then the problem is with the thermocouple and the way it is placed in it's environment.

To check that the cold junction compensation and thermocouple are working:

•Heat the joined end of the thermocouple and check that the temperature reading increases. Heat the terminal ends and check that the temperature decreases. If the temperature movement is in the wrong direction, then reverse the thermocouple leads . Heat the cold junction reference (only possible on nonisothermal block boards) and check for temperature increase. •••

CALENDAR

MIT Cold Fusion Day
January 21, 1995
Cambridge, MA

American Chemical Society
April 3, 1995
Cold Fusion Poster Session

ICCF5
April 9-13, 1995
Monte Carlo, Monaco

Society for Scientific Exploration
Annual Meeting
June 15-17, 1995
Huntington Beach, CA

The 2nd World Hydrogen Summit
September 28
-October 1, 1995
Montreal, Quebec, Canada

ICCF6
Spring or Summer/1996
Beijing, China

1996 International World Energy System Conference
June 19-21, 1996
Toronto, Canada

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Cold Fusion in a "Ying Cell" and Probability Enhancement by Boson Stimulation

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ABSTRACT

A new theory suggests that incident bosons other than photons may also enhance the probability of certain decay channels through stimulation. By applying this theory, it is possible that reactions that would normally proceed very slowly because of their small probability of occurrence could be stimulated and amplified to observable or even useful levels. The application of this theory has produced successful cold fusion on demand.

RECENT HISTORY

Paneth and Peters [1, 2] observed hydrogen in glass producing a trace of helium. Frank [3] and Sakharov [4] postulated that fusion of deuterium and deuterium can result if the electron is replaced by a muon. Alverez [5] observed the above by introducing muons into a deuterium gas, although he concluded later that the process is too slow to produce meaningful amounts of energy. On March 23, 1989, Pons and Fleischmann announced at a press conference and later published [6] that they had been able to obtain positive energy gain from cold fusion. Jones et al. [7] published in *Nature* on April 17, 1989 indicating that they had also discovered cold fusion, however, with much less energy generation than Pons and Fleischmann. Other institutions tried to duplicate the results, and some, like Texas A&M, Moscow University, and Kossuth University in Hungary, had positive results initially, while MIT et al. did not. Even those who did get positive results seemed to have difficulty repeating the success, while no one was able to obtain cold fusion on demand.

INTRODUCTION

In 1989, one of us (Ying) postulated that in as much as experimental results indicate that cold fusion exists, albeit with a very small theoretical probability of occurring, the fact that observable results are seen implies that there could be an enhancing mechanism. Ying used a model which replaced the consideration "that there is a small probability of deuterium-deuterium fusing into helium, " with the consideration "that in probability space, the channels such as Helium-4 + gamma (23.8 MeV) actually do exist [8] but occupy a very small volume of this hypothetical probability space." This would be equivalent to these states existing in the real world for vanishingly small periods of time — so short as to be unobservable under normal circumstances.

This can be thought of as being similar to the condition that exists in a laser, where the lasing medium, having been energized, has the potential of emitting photons of a certain frequency and energy level. Like the lasing effect, however, a specific type of stimulus must be applied to cause this to happen. Otherwise, there is only a much lower probability that such a spontaneous reaction will occur.

Probability space can be thought of as all possible states that a system may occupy. This space can be defined as having n dimensions, based on the constraints of the system being modeled. Some of the states that can exist in this space are highly probable and can easily be reached with little or no influence on the system. Other states are not very probable, yet the uncertainty principle allows us to postulate that these state do indeed exist. These states, however, occupy extremely small niches in probability space, due to their low probability. Probability space is a model similar to phase space. Phase space is such that for n unconstrained particles, there will be a total of 6n dimensions, three for position coordinates and three for momentum coordinates for each particle in the phase space. Phase space can become very complicated rapidly, as estimates show that for a room full of air molecules, the phase space might have roughly lx10^28 dimensions.

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For any system, one can model a probability space that has points that correspond to any possible configuration of the system. The difference between phase space and probability space is that probability space can be much simpler, because probability space maps only the states a system may occupy, with their volumes related to their probabilities, whereas phase space also shows how many different ways that a certain state could be reached. Consider the complexity of a phase space with a vector field imposed on it, representing time-evolution of the space according to Hamilton's equations.

Based on this model, Ying theorized that there could exist boson stimulation mechanisms to cause a short-lived or virtual state to precipitate into reality. In effect, this is a method of enhancing the probability that a particular reaction will occur, taking the virtual state out of his mathematical probability space and into normal space. In the case of cold fusion, the decay channel state of interest is "fusion having occurred."

Using the lasing model to illustrate this, a photon introduced into the energized lasing medium can trigger the formation of other identical photons. When this happens, the potential for such a photon to exist is actualized, and in Ying's probability space model, this is equivalent to having influenced the probability of this event occurring. In a lasing medium, of course, the emission of the photon from the medium, because of the passage of another photon, is due to the symmetric nature of the wave-function under the exchange of the two photons. One photon is "pulled out of" a virtual space or a probability space by the incident photon.

Inasmuch as alpha particles are bosons and one of the decay channels of our experiment involves an alpha particle, the act of providing an incident alpha particle onto the interaction site can also stimulate the emission of an alpha particle in the same state. The act of producing this alpha then determines that the fusion must have occurred, otherwise no such alpha could have been emitted. The result is that a fusion event takes place and energy is released as gamma rays. Other possible mechanisms exists, in part due to the fact that the reaction substrate is a metal rod or foil. Coupling to the metal atoms as a whole could result in energy other than gamma rays. This is referred to as coupling to the lattice [9].

Applying this theory to cold fusion requires an experimental setup (Fig. l) consisting of some heavy water, an inert anode and a cathode which attracts and holds hydrogen, and a battery. Platinum metal was selected for the anode, and palladium was selected for the cathode, although experiments suggest that other metals such as nickel or titanium might work as well. The battery is used to supply electrolysis current to the electrodes, thus allowing the heavy water to be split into deuterium and oxygen. The deuterium liberated is the fuel for the reaction.

New palladium electrodes require from 1.5 to 2 hours of 'seasoning' time to load enough deuterium into the metal lattice. Then incident bosons are aimed at the target in the experiment. In this case, the target is the palladium cathode. The "Ying cell" is placed inside a Faraday cage (which was done after run #55 when Shults was burned on his finger, under his ring, at a distance of 1.4 meters from the operating cell— apparently by some unknown emission of electromagnetic radiation), and the entire apparatus is placed inside a double-insulated container. When the bosons 'illuminating' the target are the same as at least one type of boson that a successful reaction could emit, an identical boson can be created via stimulation, forcing the reaction to proceed and resulting in nuclear fusion. This effectively amplifies the probability of fusion occurring, and can give a net result of observable energy. This effect could potentially be scaled up to produce a useful quantity of power.

Consider the case of introducing 23.8 MeV gamma rays at the target. The expected result, if we apply this theory is the generation of He-4 and more 23.8 MeV gamma radiation. This is cold fusion. But this also implies the possibility of using alpha particles (which are helium-4 nuclei) to stimulate the process, resulting in more helium-4 and 23.8 MeV gamma rays. Once again, this is cold fusion. Ying notes, however, that cold fusion is only one example of his general theory of using incident bosons to enhance probabilities.

In December of 1991, we (Ying and Shults) teamed together to pursue the question of feasibility of cold fusion through this process. One- hundred-and-two experiments later, we finally have results supporting several successful experiments, using several different cell geometries. We were trying to show that we could indeed initiate cold fusion on demand. All the geometries we considered used a straight 1.0 mm diameter Pt wire as the anode. For the cathode, cell #1 used a straight Pd 1.0 mm diameter wire, cell #5 used a coil of the 1.0 mm diameter Pd wire as a cathode as shown in figure l, and cell #6 used a 2.5 cm x 2.5 cm, 0.025 mm thick Pd foil as a cathode. Other cells were either duplicates or control dummy cells. It turned out that the geometries are not important to the theoretical basis of the experiment, but are perhaps useful in future attempts to scale the effect up.

Four thermocouples are used in the experiment. Two are taped to the exterior of the fusion cell at different heights. The other two are placed outside of the Faraday cage but within the insulated container. This placement was done to prevent incorrect readings due to potential hot spots.

During the experiment, cells sometimes showed a decreasing ability to produce heat. We experimented with methods of rejuvenating these cells, and found that often, applying a reverse voltage to the cell and allowing it to set overnight would cause the cell to begin to react again. This is easily accomplished by swapping the anode and cathode leads. A power of 15 volts DC is normally applied for the experiments, and reversing this supply for about 10 seconds is usually sufficient to cause the cell to become operational once again. No electrolyte salt was ever added to the heavy-water.

Conclusion

Ying's theory of "using incident stimulation bosons to grab decay channels out of probability space into normal space, thus enhancing probability" predicts, among other things, that cold fusion is possible on demand by stimulation. Following this reasoning, the present set of experiments was initiated, for the purpose of either proving or disproving the theory. This series of experiments showed a much larger heat production than can be accounted for by ohmic, chemical, or charging mechanisms. When deuterium oxide is replaced with distilled water or when alpha or gamma stimulation are removed, we do not get the "Ying signature" heat rise (Fig. 2). The success in obtaining 2 to 4 orders of magnitude power increase, production of excess heat, and cold fusion on demand in a "Ying cell," when deuterium oxide and alpha and gamma are all present, is an experimental proof of Ying's theory.

(Contiued on page 49)

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Japanese Television Program Reveals Industry-Government Program in Energy from Magnetic Materials

In what must surely be one of the most unusual disclosures of a claimed scientific-technological development, Japan's Fuji Television Network aired an eight-minute program on so-called "Dream Energy" on October 20, 1993. This program has only recently come to light in the U.S. Mechanical engineer Bertil Werjefelt of PolyTech(USA) in Keneohe, Hawaii, maintains that this Japanese research confirms work that he has carried out on "energy from magentic materials and magnetic fields" over the past decade or more. He provided *IE* with this transcript of that Fuji television program. The English translation was provided by Professor James Kenney of theDepartment of Japanese Studies at the university of Hawaii:

"The Dream Energy"

Newscaster Woman's Voice:
It's been said that its impossible to gain usable energy from permanent magnets.

Newscaster Man's Voice:
But it's been discovered that it's indeed possible to extract energy from permanent magnets; and our staff has pursued this matter to reveal to you the secrets of this new technology.

Script on Screen:
[Professor at Waseda University, Engineering Department (Physics) Prof. Yoshihiko Otsuki]

Otsuki:
This is something that can turn the whole 21st century around - in terms of new energy. When I visited and saw the experiments in action my hands literally trembled from excitement.

Script on Screen:
[(Japan Science Research Laboratory) = Nihon Riken Mr. Teruhiko Kawai]

Narrator Woman's Voice:
What had surprised Prof. Otsuki was the fact that there was a high energy ratio achieved; that there was, in fact, a ratio of over 100%. (The phrase used is "more output than input.")

Script on Screen:
[Meiji University Scientific Engineering Department (Engineer) Prof. Masao Mukaidone]

Mukaidone:
What we found were some very interesting facts.

Narrator Woman's Voice:
Permanent magnets have the nature to repel and pull. Prof. Kawai has succeeded in making permanent magnets produce mechanical energy. To convert magnetic energy to mechanical energy.

Script on Screen:
[Now Revealed: Dream Energy]

Narrator Woman's Voice (& Script on screen):
[Tokyo,Kamata]
All he ever thought about was permanent magnets. And one day, he discovered the principle that magnetic energy can be converted to mechanical energy.

Script on Screen:
[Experimental Equipment]

Narrator Woman's Voice:
Was this really possible? The first experiment was about to begin.

Script on Screen:
[Prof. Kawai]

Kawai:
I was very anxious... Was it really possible? Will I be able to obtain usable mechanical energy?

Script on Screen:
[Jan 12, 1991 - this fact was determined]

Man's voice:
We placed the permanent magnet - more turns were observed as well as torque.

Narrator Woman's Voice:
Yes, it was indeed possible to obtain more mechanical energy...

Kawai:
I was extremely happy with the results.

Narrator Woman's Voice:
Let's have Prof. Mukaidone, who has been with this experiment developed by Prof. Kawai from the beginning, comment on it.

Mukaidone:
What we have here is the more effective employment of switching - - of turning on and off of magnets in a sequence.

Script on Screen:
[The Principal of Switching]

Mukaidone:
Same poles attract, only the opposite poles repel. Here, we have two magnets with a piece of iron between them. We turn the pole on one magnet, and the piece that was picked up, falls.

Narrator Woman's Voice:
Two magnets have a piece of iron between them. Then, this piece too becomes magnetized and can attract another piece of iron. Switch the pole on one magnet, and this piece drops.

Script on Screen:
[October 12, 1993 - This principle is made public]

Man's Voice:
Let me explain using a diagram. Here we have a motor with a permanent magnet imbedded, with protruding magnets placed around its frame. With the core magnet, those individual magnets are magnified and through switching these individual magnets, the motor turns.

Narrator Woman's Voice:
With sequential switching of the external magnets, the motor turns and increases its speed. This principle can be applied to both direct and alternating current motors.

Script on Screen:
[Staff Member: Isamu Fukui]

Narrator, Woman's Voice:
We place a very powerful permanent magnet into the rotor. This is a direct current motor employing the permanent magnet principle.

Script on Screen:
[direct current motor]

Narrator Woman's Voice:
What we had here was an output exceeding 100%. Seeing the results, the professors were amazed.

Script on Screen:
[June 28, 1993 at Nihon Riken]

Kawai:
Where did this extra output come from? This is really a revolutionary find!

Mukaidone:
This can only be attributed to an extreme-

New Energy Times Archive

ly higher level (magnitude) of electron levels.

Narrator Woman's Voice:
Now we begin an examination of this extraordinary motor. In February the engineers of the Sumitomo group examined the motor.

Mukaidone:
The turning level is very high...

Narrator Woman's Voice:
They check to see that all data achieved is correct.

Kawai:
We checked to see same readings were gained on other meters; not just the ones we used on our initial experiments.

Narrator Woman's Voice:
Professor Kawai feels that they have laid the foundations of the principle; now, it's time for private enterprises to utilize these findings.

Script on Screen:
[Narration = Miki Aoki]

Narrator Woman's Voice:
Now, they will apply the principle to existing motors (that are out on the market).

Script on Screen:
[Experimenting with store-bought motors]

Narrator Woman's Voice:
The turning ability of the motor is increased and effectiveness level is also greatly increased; 92-98% is achieved.

Script on Screen:
[Sumitomo Aerospace Unit 2, Division Chief Hiroshi Tamagawa]

Tamagawa:
What we have here is a curious phenomenon whereby we've come up with a better motor.

Narrator Woman's Voice:
Professor Mukaidono has come to check the meter reading and data.

Man's Voice (Mukaidone):
We're seeing what levels can be achieved with existing motors using our principle. We're now showing .66 amps. Turning (RPM) is 1500 and shows no change

Script on Screen:
[Prof. Mukaidone}

Mukaidone:
Same amps and same voltage... seeing this, what we have here is a motor that we've never seen before. With better output, we'll have a better, more efficient motor. If motor builders take a look at this, they'll be just amazed!

Narrator Woman's Voice:
Prof. Otsuki has also arrived to take a careful look.

Script on Screen:
[Prof Otsuki]

Otsuki:
I sure am surprised. The utility rate is up by 15 -20%. Using conventional oil-based energy, the best one can expect is an energy savings of 5%. This is dramatic energy savings - perfect.

Script on Screen:
[Magnet- Powered car]

Narrator Woman's Voice:
The principle can be applied to running cars, trains, and also the running of household appliances. The energy is highly efficient, economica, and very clean.

Script on Screen:
[Prof. Kawai}

Kawai:
We've discovered that these secrets were present in our surroundings (nature) all along... we can now use it to help restore the environment that man has been abusing.

Newscaster Man's Voice:
This is good, clean energy ...amazing!

Newscaster Woman's Voice:
And, there's no waste materials produced...truly the source for cleaner, more efficient energy for the future.

(Continued from page 47)

Epilogue
Since the original successful experiment and press conference and television appearance in June 1992, we have been trying to build more useable cells which can give us 'cold fusion' on demand. Our effort showed the foil geometry to be the best. We have had successful cells using palladium cathodes from both Johnson Matthey and Wilkinson. The foils we used are 0.1 mm thick and .999 pure.

With our ability to create more useable cells which can produce fusion on demand, we can now assist other researchers who up to now have not been able to reproduce our work by following our verbal descriptions. Earlier this year we called a meeting of cold fusion researchers. We will call another one later. We can accommodate a limited number of guest researchers. To a very few of them, we can even give one of our working cells. Interested researchers are invited to express their desire to be included in the next round of invitations by writing to Dr. Nelson Ying of Balquhain, Principal Investigator, Quantum Nucleonics, Box 22956, LBV, Florida 32830.

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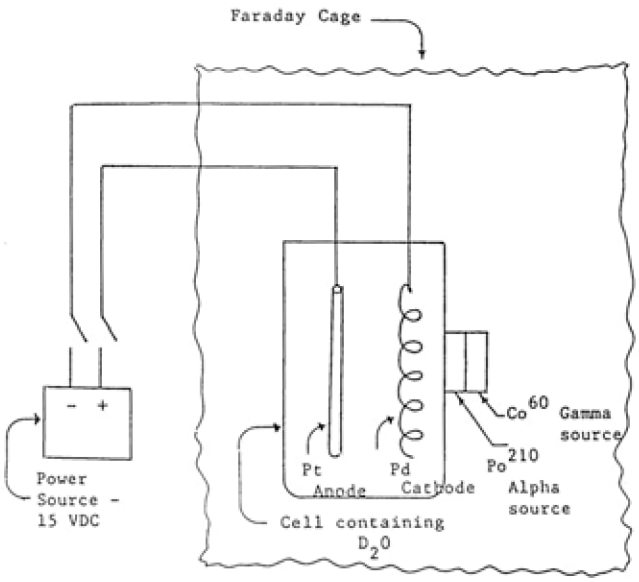

Figure 1: Experimental setup for the 'Ying Cell'.

1. Figure 1: Experimental setup for the 'Ying cell'

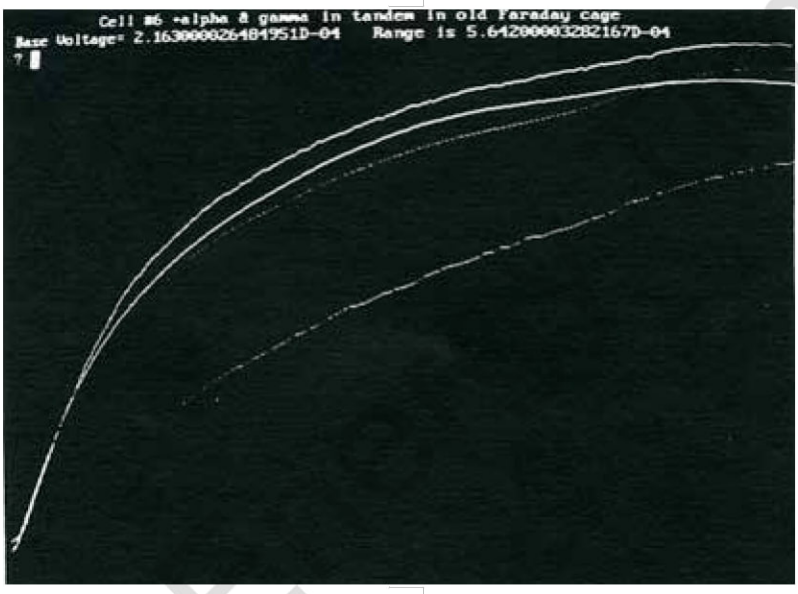

Figure 2: Temperature increase as a function of the time for cell #6, Run #102. In this run, the alpha and gamma sources are in tandem. The y-axis is temperature showing 0.5 degree C per division. Here we have over 12 degrees C increase. The x-axis is time, with 275 seconds per division. Here it shows that the 12 degrees increase occurred over a one hour period. The four lines represent four separate thermocouples placed in different locations close to the 'Ying Cell,' with two directly on the cell on opposite sides and the other two outside of the Faraday cage, but inside the insulated container.

Washington Watch
— A Letter from the U.S. Senate

January 25, 1995

Dear Dr. Mallove:

Thank you for passing along your opinions and concerns regarding fusion energy technology which I have reviewed. I appreciate you enclosing a copy of the testimony on cold fusion energy submitted by you and your colleagues to the House Science, Space and Technology Committee in May of 1993 and the opening statement by the Honorable Robert Walker from this hearing.

I share your concerns that federal funds should not be wasted. I appreciate you taking the time to explain your views supporting cold fusion, and I will certainly consider your concerns on tokamak hot fusion development as the Republicans look for ways to cut federal spending during this session of Congress.

In the past, I have supported elimination of funding for wasteful energy projects. In 1983, while in the House of Representatives, I fought to eliminate funding for the Clinch River Breeder Reactor because of economic and environmental concerns. Its successor, the Advanced Light Metal Reactor (ALMR), possessed many of the same economic and environmental problems that led to the cancellation of the Breeder Reactor. On September 29, 1993, and again on June 27, 1994, I sponsored amendments to eliminate funding for the ALMR program; unfortunately, these amendments failed to pass, however, the ALMR was terminated in the House/Senate Conference on the FY 1995 Energy Appropriations bill which was signed into law on August 26, 1994.

I believe the size and scope of the federal government is too large. America can no longer afford to have Congress spend the taxpayers' money with no accountability. Congress needs to put political expediency aside and concentrate on reducing our budget deficit and serving the public that elected them. Last year, I received several awards from both national and grassroots groups that track votes cast by Members of Congress which relate to federal spending. I was recognized by both Congressional Quarterly and the National Taxpayers Union as the Member of Congress who most often voted to cut federal spending. My ranking as Congress' most frugal Member comes from my strong belief that if we are to control our national debt Congress must curb spending. Additionally, Citizens Against Government Waste

(CAGW) has recognized me as the Senate's only "Taxpayer Hero." This is an honor which I worked hard to achieve. CAGW continues to shed light upon an issue that, for many years, went unchecked -- excessive government spending. I believe reducing the size and cost of the federal government is the only way to achieve real deficit reduction. Raising taxes on the American people will do little to lower our national deficit. Congress must repress past spending habits and make the tough decisions that will guarantee the next generation is afforded the same opportunities we were.

Again, thank you for contacting my office. I appreciate you bringing this issue to my attention and if I can be of further assistance to you, please don't hesitate to get back in touch with me.

Sincerely,

Judd Gregg
U. S. Senator
New Hampshire

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For an Excellent Overview and Introduction to Cold Fusion and New Energy Technology.....

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at the Massachusetts Institute of Technology

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Dr. Eugene F. Mallove
(Cold Fusion Technology, Concord, NH)
"Cold Fusion and New Energy Technology - An Overview"

Dr. Peter Graneau
(Northeastern University Center for Electromagnetic Research)
"Anomalous Forces in Water Plasma Explosions"

TAPE#2

Buford ("Ray") Conley
(MIT, graduate student, Dept. of Aero/Astro Eng.)
"Light Water Excess Heat Experiments"

Fred Jaeger
(ENECO, Salt Lake City)
"Patents and Commercialization"

Professor Peter L. Hagelstein
(MIT Dept. of Electrical Engineering and Computer Science)
"Cold Fusion: What We Know and What We Don't Know"

TAPE#3

Professor Peter Hagelstein
(CONTINUED from TAPE #2)
"Cold Fusion: What We Know and What We Don't Know"

Professor Keith Johnson
(MIT Dept. of Materials Science and Engineering)
"Progress in the Theory of Excess Heat" *and Excess Heat*- The Movie, now being made by Hollywood!

James L. Griggs
(Hydro Dynamics, Inc., Rome, Georgia, USA)
"The Hydrosonic Pump: Excess Power and Other Effects"

TAPE#4

Bertil Werjefelt
(PolyTech(USA), Hawaii)
"Energy from Magnetic Materials and Magnetic Fields"

Dr. Eugene F. Mallove
(Cold Fusion Technology, Concord, NH)
Concluding Remarks

The tapes include the speakers' presentations as well as question and answer sessions. There is footage of the Hydrosonic Pump under test as well as close-up footage of rotor surface melting effects. Bertil Werjefelt shows footage of experiments in his own laboratory. There are scenes of Japanese researchers from Sumitomo, Hitachi, and other companies and universities now involved in their claimed extraction of energy from magnetic materials. These efforts are now said by these researchers to be proceeding toward commercialization. Actual experiments are seen in progress. The tapes are VHS cassettes in NTSC format and range from 120 to 127 minutes each.

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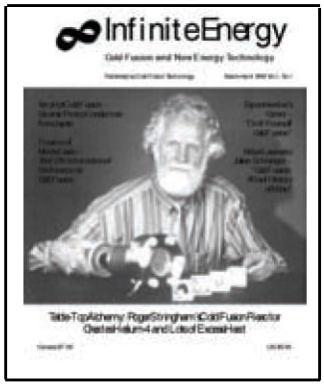

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